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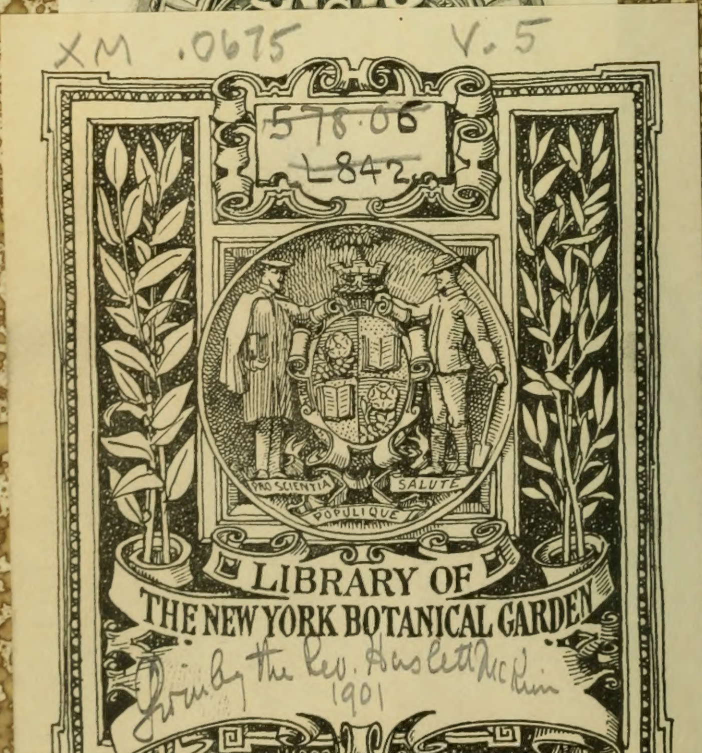

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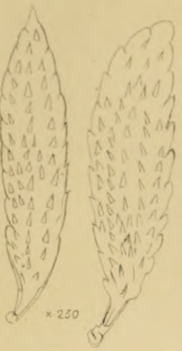
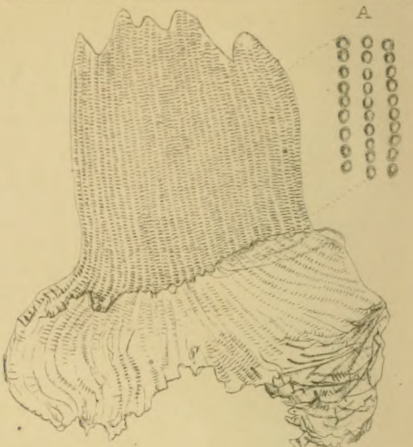
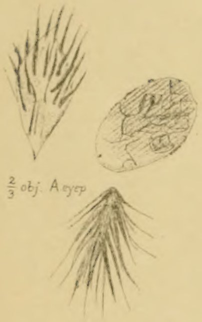


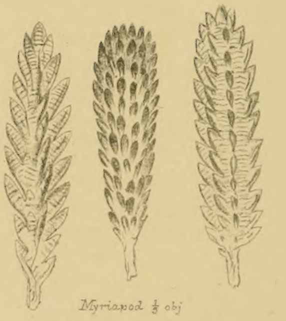
Scales of Scenicus Saltians.



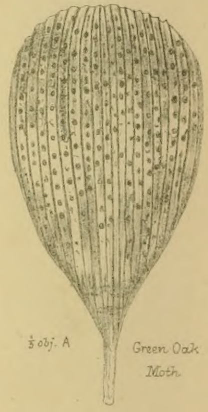
Scale of Butterfly torn open.



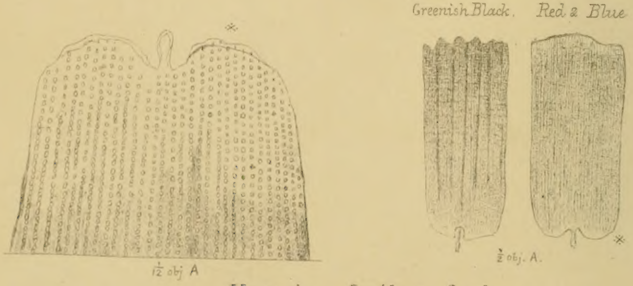
Hypomeces Squamosus.
Scales.



Polyxenus Lagurus



Procris Statices.



Urania Leilus – Scale.

J. McIntire del. T. West sc.

W. West & Co. imp.

Insect Scales.

THE MONTHLY
MICROSCOPICAL JOURNAL:

TRANSACTIONS
OF THE
ROYAL MICROSCOPICAL SOCIETY,
AND
RECORD OF HISTOLOGICAL RESEARCH
AT HOME AND ABROAD.

EDITED BY
HENRY LAWSON, M.D., F.R.M.S.,
Assistant Physician to, and Lecturer on Histology in, St. Mary's Hospital.

VOLUME V.

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Monthly Microscopical Journal, January 1, 1871.

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The Monthly Microscopical Journal is furnished free to Members of the Royal Microscopical Society. Non-Members can get it from any bookseller, price 1*s.* 6*d.* per month.

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THE DEATH OF OUR PRESIDENT.

It is with the deepest grief that we have to announce that our President, the Rev. Joseph Bancroft Reade, M.A., F.R.S., F.R.A.S., is no more. On Monday morning, the 12th of December last, he passed from among us in the seventieth year of his age—passed with regret that he should be compelled to leave us, and yet with calmness, courage, and confidence in the Future. Those who knew him throughout his long and active life can say more of his many doings than we can now—can record a career of noble activity. But of his manly nature, his child-like simplicity, his endearing disposition, his appreciation of those who surrounded him, and his thorough kindliness of disposition, we cannot speak in terms too high; or be even momentarily accused of leaning too deeply on the many virtues of one whom, in common with all who knew him, we mourn the loss of with a grief too deep to be fully expressed. Hereafter his life will be told by one who has loved him well, and who knew of all that he had done and much that he had thought. For the present we may say, as has well been said on another occasion, the fewest words are best where all words are Grief.

LIBRARY, NEW YORK BOTANICAL GARDEN.

THE
MONTHLY MICROSCOPICAL JOURNAL.
JANUARY 1, 1871.

I.—*Note on FLUORESCENCE* v. *PSEUDO-DICHROISM.*

By the late Rev. J. B. Reade, F.R.S., P.R.M.S.

(*Read before the* Royal Microscopical Society, *Nov.* 9, 1870.)

At several meetings during the last three years I have exhibited to the Society a coloured liquid, prepared by Mr. Sheppard from fresh-water algæ, species undecided, found in a spring in Kent. An exactly similarly-coloured solution was described by Dr. Ferdinand Cohn, of Breslau, in the number of Schultze's 'Archiv,' published about the same time, early in 1867.

The characteristic qualities of the English liquid will be found detailed at length in a letter from Mr. Sheppard to myself, read before the Society on the 8th of May, 1867; and I will here quote Dr. Cohn's description, which so exactly tallies with that of Mr. Sheppard, that we may assume that the liquids are exactly similar, and that the phenomena of colour are the same in both.

Dr. Cohn writes:—"Another most startling optical peculiarity of the Phycocyan is its *fluorescence;* for seen only by transmitted light on a clear background, the Phycocyan appears indigo-blue; but with reflected light and a dark background it shows an intense carmine red." This account of the play of colour is exactly that given by Mr. Sheppard, except that he compares the colour seen by reflected light with that of the carnelian; and I think that this simile is more exact than that of Dr. Cohn, seeing that the liquid shows an opaque dull intense scarlet, rather than the bright transparent crimson of carmine.

Now this phenomenon of fluorescence (for Dr. Cohn's phrase has been generally adopted) has attracted less attention than might have been expected. Other solutions, for instance quinine, exhibit fluorescence, or faint iridescent gleams of blue and pink; but in the case of Phycocyan the violent change from *all* blue to *all* red by altered illumination goes far beyond what is seen in any of these chemical compounds, and leads one to believe that the one name Fluorescence is not a proper description of the two sets of appear-

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ances. I hold the play of colour in the two cases proceeds from two very different causes; and that this opinion is correct I shall now be able to show by an experiment. I have here a bottle containing water stained blue by a little indigo from my water-colour paint-box: to this transparent blue liquid I have added a little vermilion in impalpable powder; and straightway I have obtained all the fluorescent qualities of Phycocyan. That is, two colouring matters, one opaque the other transparent, dissolved *out of* the plant (according to another theory *developed from* the plant), are represented in one artificial liquid by indigo and vermilion. Now hold the bottle to the light, the transparent indigo alone is visible, the grains of vermilion showing only their shaded sides, which appear grey or black by contrast with the strong light, and therefore, do but subtract a little brightness from the blue. Now change the position of the bottle, and by so doing change the illumination; the "intensiv karminroth" of Cohn at once shows itself. The transparent blue throws back no coloured rays, but the opaque red particles are exhibited in quantity sufficient to make "the *blue* one red" (Shakspeare). Seeing then that the fluorescence of quinine and this fluorescence of Phycocyan spring from different causes, I think it will be proper to distinguish them, and I propose for shortness to name this play of colour described and exhibited to-night—*Pseudo-dichroism.*

I am indebted to my friend Mr. Sheppard for the ingenious *experimentum crucis* above described. It is the exact counterpart of nature's operation in the production of *Phycocyan*, which consists of opaque red particles in a transparent blue fluid. Thanks to my friend for his solution of the mystery, and also for enabling me to present this account of it for the consideration of our Fellows.

"Omne ignotum pro magnifico."

But the mystery ceases when we know all about it.

BISHOPSBOURNE RECTORY, *Nov.* 7, 1870.

II.—*Notes on the Minute Structure of the Scales of Certain Insects.*

By S. J. McIntire, F.R.M.S.

(*Read before the* Royal Microscopical Society, *Nov. 9th*, 1870.)

Plates LXIX., LXX., LXXI.

Dr. Pigott's paper on High-power Definition has caused me to devote some further attention to the markings of the scales of insects; and as some of the results I have obtained in the examination of a very great number of scales from all sorts of insects are curious, it may be well to place them on record, since they may prove as novel to a few readers as they were to me.

The first fig. on Plate LXIX. represents the scale of one of our hunting spiders, *Scenicus salticus*, the prettily-marked zebra-spider, which has so often furnished a theme for natural history writers, from its ingenuity in laying wait for and capturing its prey by an accurate and sudden spring upon it—a process which may be easily seen on any hot summer day on walls and palings fully exposed to the sun. The scales are situated on all parts of its body and legs; and with some care, though not without difficulty, they may be detached and examined under high powers. Their margin is beautifully crenated, and the outer membrane is smooth, while the inner membrane—that is, the membrane next the spider's body—is puckered up into somewhat irregular rows of hackles. When I first found this scale I was struck with its resemblance, though a distant one, to the view which Mr. Beck entertained respecting the surface-contour of the Podura scale (Lepidocyrtus). I could not help regarding it as, to a certain extent, confirming his view that the "interjection markings" on that scale are elevations of the membrane. I am still further strengthened in the notion since reading Sir John Lubbock's latest observations upon the position which the Thysanura occupy in relation to entomological classification. It would appear, from the absence of tracheæ in many of these creatures, the presence of certain abdominal appendages (possibly representing extra legs), the absence of metamorphosis, and some few other points, that this authority is disposed to see in them a closer relationship to the Arachnida and Myriapoda than to the Insecta. At all events, it is clear that the peculiar hackle-like structure, which has been asserted for the Lepidocyrtus scale, is not without a parallel among the tegumentary appendages of the Articulata.

Another very curious scale I found surrounding the eyes, and various portions of the under-surface of one of the Chinese Curculionidæ, *Hypomeces squamosus*.* This scale is leaf-shaped; its surface is also hackled, somewhat like that of the spider-scale I have alluded to, but to a much greater extent. Perhaps some will regard it as plumose. It also presents the phenomenon so common to many insect scales, of decomposing the light which passes through it, so that some portions of its fringes display gorgeous colours by transmitted light,—colours more or less complementary to those seen in the same scale by reflected light.

---

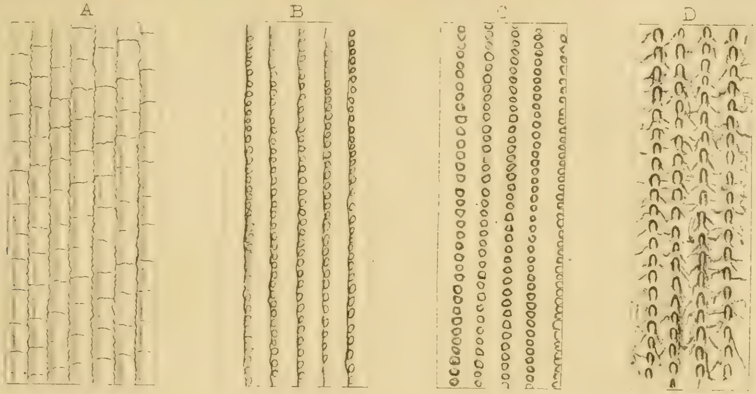
* It would perhaps be correct to consider these two scales as imitating the structure of compound hairs.

In the scales of *Polyxenus lagurus* I have found what is very uncommon (so far as my examinations, which have been very carefully made, are reliable), a deposit between the membranes. The scale is a solid structure, not, as in the generality of cases, merely an upper and lower membrane without any intermediate deposit. The surface-contour is uneven, swelled into elevations at regular points.

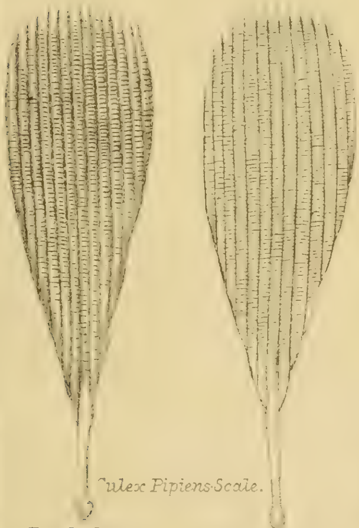
The presence or absence of a beaded deposit between and distinct from the membranes of scales has formed a branch of my inquiry, and I endeavoured to satisfy myself upon the point by tearing open or crushing between glasses such specimens as showed the phenomenon of beads in a marked manner by ordinary illumination. The only results I obtained on these occasions were negative. One of the most marked examples I have endeavoured to represent. It is from one of the exotic Argynnidæ, but I am unable to name the butterfly whence I got it. The scale was torn in such a manner as to display the interior of the upper and lower membranes. There was no débris in its neighbourhood resulting from the escape of any fluid matter or beads, and the damaged membrane only gave evidence that its folds or corrugations were destroyed. The colour of the scale was deep yellow. In all other similar experiments, except where I had actually pulverized the scales in my efforts, similar negative appearances were presented.

Thus, the investigation led me to the conclusion that whatever beads (with exceptions to be noticed presently) I saw in the scale, before submitting it to injury, had no real existence as *beads*, but were due to the interference of the rays of light by corrugated membranes; they were ghost-beads, in fact.

A still more striking example of these ghost-beads or illusive appearances is given in the iridescent scales of the beautiful West Indian moth, *Urania Leilus*, which, like those of the Diamond beetles, display chromatic effects by transmitted as well as by reflected light. When one of the semi-transparent scales is taken (say one of those orange-red by transmitted, and metallic-green by reflected light), and the surface next the object-glass accurately focussed, an appearance like A in the illustration is seen; but if the focus be altered so as to penetrate into the scale, views resembling B, C, and D are given (Plate LXX.), according to the depth to which we try to penetrate our glance into the interior of the scale and the quality of the illumination we use.

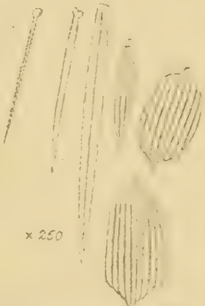


Iridescent Scale of Urania Leilus, shewing Pseudo-beads

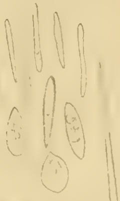


Culex Pipiens Scale.

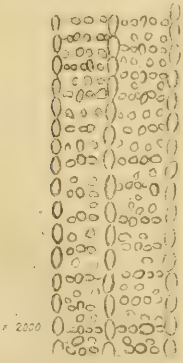
Pseudo-beads. true appearance.



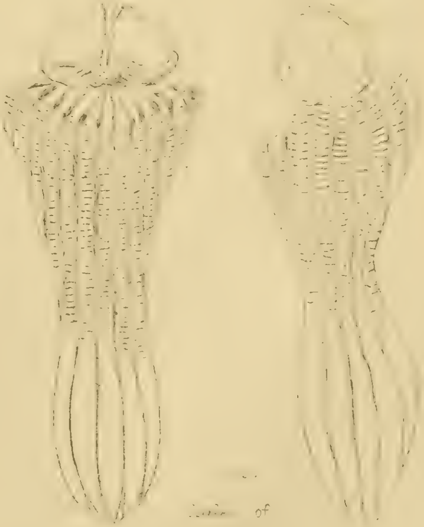
Scales & hairs from [illegible] of Attagenus Pellio



Iridescent hairs & Scales from Cyphus Germar
× 30



Portion of ordinary Scale of Pieris Epicharis, shewing Pigment Cells.



[illegible] of Pieris Agathina.

J. M^c Intire del. T. West sc
W. West & C^o imp

This experiment is a very curious one, and has occupied a great deal of my attention. Sometimes I have succeeded, without any difficulty, in satisfactorily proving to myself by rapidly calling into existence, and as rapidly dismissing these almost palpable beads, that they were mere illusions; and at other times, when I have wished to do so, I have found it impossible so readily to call them up, or to make them disappear if already in view. These failures, I believe, were due to a predisposition in my own mind at the time to recognize one view more strongly than the other, rather than to want of skill; though no one knows better than myself that the necessary adroitness in manipulating high powers is not always at one's own command.

Another curious example of breaking up the surface of a scale into false beads at pleasure was afforded by the inflated plumule of *Pieris Agathina* (Plate LXX.). (I have drawn it under conditions which seem to me to yield a truthful view.) On raising and lowering the achromatic condenser, and varying the illumination, some remarkable effects were presented,—now the longitudinal ribs assumed a beaded appearance, and directly afterwards the lateral ones resolved themselves each into three or four similar false images.

A similar experiment was tried with the scale of the English mosquito, *Culex pipiens.*

These and many other experiments, especially the examination of scales which lie across each other at various angles, causing distinct and palpable beads where the ribs intersect (an optical effect familiar to most of us), lead me to hold the opinion very strongly that very much of the beaded appearance we see on scales (*how much* I would not venture to say) is illusive, and very often caused by the "dodges" we resort to in illuminating them.*

As regards an *internal deposit* in scales, suggested to be the case in the article in 'The Student' for February on the Scales of the Lepidoptera, I venture to dissent from that conclusion; and perhaps I may be permitted to state why, theoretically.

Such a deposit would, I think, considering the purely protective, and sometimes only ornamental, functions of scales, be of neither use nor ornament. It would detract from their elasticity and lightness, while it would add to their weight. Its presence could only, I think, be atoned for by some great necessity like that of the bulb and medulla of vertebrate hairs, which is a store of growing materials, needed by the slow and gradual growth of these structures. But insect hairs or scales—for no one doubts these are closely analogous, if not identical structures (I furnish a few examples of transitional forms which might be multiplied *ad libitum*)* —are fully developed before they are exposed to external influences at all,—*i. e.* from the moment when they become external appendages they do not grow. So that I cannot see the necessity, as a rule, for any growing matter in their interior. There are some hairs or spines on the legs of spiders, I know, which form sheaths for new ones, but I do not refer to these: they are more than mere hairs. Let anyone, by way of experiment, examine carefully the hairs of caterpillars, or the more delicate hairs of spiders, and I think he will find them to be dry membranous hollow tubes, often of simple structure, and quite as often very complex.

---

* A lurking suspicion exists in my mind that some of these "dodges" produce results approximating to those which Dr. Pigott obtains. For instance, for the last six or seven years I have been acquainted with the so-called beads upon the scales of *Degeeria domestica* and *Macrotoma major;* but I have not hitherto interpreted them to be other than corrugations, *vide* 'Science Gossip' for 1867, Figs. 43, 44, and 50, where the scales are figured.

Believing as I do that the Podura beads, which are just now so eagerly looked for by microscopists, partake very much of the nature of the illusive appearances I have so far alluded to, I nevertheless do not decry such appearances. Their cause resides in the scales and not in our object-glasses; in fact, I think the better our object-glasses are, the better they will be shown, provided always that we illuminate the scales in the manner required to throw up these beaded shadows. We see them with the same mixture of delight and incredulity that we applaud the performances of Pepper's *Ghost*.

But there are other beaded appearances in scales, the causes of which we can better understand.

Let us take any Lepidopterous scale, possessing much colour and opacity. Let us illuminate it with a very strong light, and bring high enough amplification to bear upon it; and we shall find that the substance† which causes the opacity and colour of the scale is granular, and for the most part of regular circular or elliptical outline in its minute details. It seems to reside upon, or very near the surface of the membrane, which it renders more or less opaque, without, so far as I can discover, increasing its thickness. In describing the scale of *Amathusia Horsefieldii* some years ago in the 'Microscopical Journal,' Mr. Warren De La Rue fixed his attention more particularly upon this one feature of scale-structure, and gave an account of it which is, to my mind, quite satisfactory. He considered these granules to be pigment, and spoke of them as such. Since his paper is largely quoted in Quekett, and a copy of the original drawing appears there too, I need only indicate where interested inquirers may read his opinions.

I think the term "pigment cells" which he uses, is a suitable one to describe these almost omnipresent granules. They are of such general occurrence, and oftentimes of such a pale tint, that I would not venture to assert any scale to be *wholly* without them, though clearly in some scales they are very numerous, and in others

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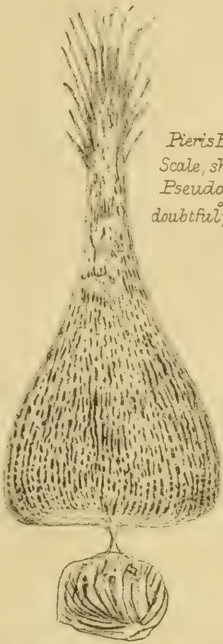
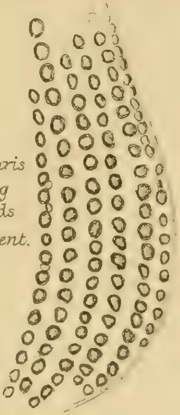
* Scales from *Cyphus Germari*, and scales from larva of Attagenus.

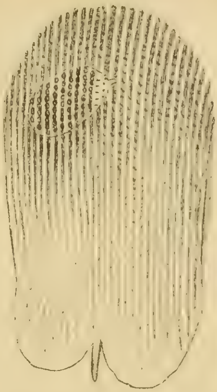
† The note by Dr. Maddox appended may afford a clue to its chemical nature.



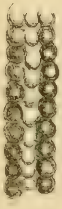
Pieris Epicharis Scale.

Pieris Epicharis Scale, shewing Pseudo beads or doubtful pigment.



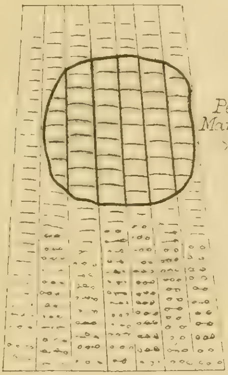


Macrotoma Major 2 Scales.

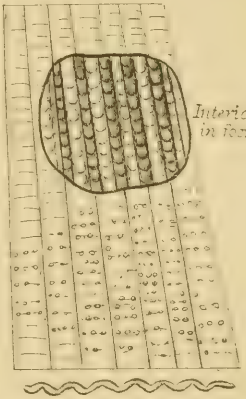


Pseudo-beads.

Petrobius Maritimus Scale

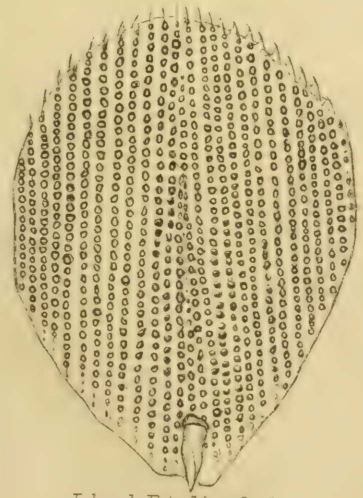


Surface in focus.

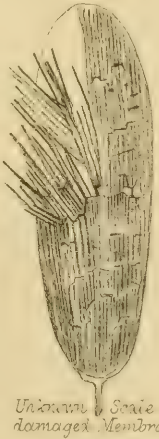


Interior in focus

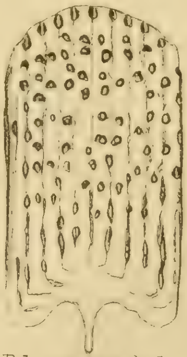
Ideal section (transverse)



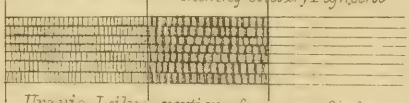
Inland Petrobius Scale shewing Embossings.



Unknown Scale damaged Membrane



Polyommatus Scale Exotic, unknown shewing solitary Pigment



Urania Leilus, portion of opaque Scale shewing Pseudo-beads or embossings

J. Mc Intire del. T. West sc.

W. West & Co. imp.

Insect Scales.

very few. The beautiful drawings illustrating the paper in 'The Student' for last February, to which allusion has already been made, strike me as being faithful representations of these *pigment* granules. I take exception, however, to those showing beads in the Podura scales, for these seem to me to represent in an equally satisfactory manner, the "ghost-beads" I have spoken of above.

Thus, provided others can confirm my observations, the existence of two causes of beaded appearances has been traced *in extreme cases;* but how far, in some scales, either of these causes preponderates over the other, will be, I believe, a puzzle to microscopists for many a day to come.*

But we are not done with the scales of insects. There yet remains another feature to be briefly alluded to, namely, the modifications of the folds or corrugations of the membranes themselves. In some scales (see the figure of the scale of the larva of Attagenus) the corrugations are such that the surface presents longitudinal grooves and ridges only. In others (and by far the greater number are of this pattern), a second set of corrugations crosses the primary set at right angles, breaking up the surface into embossings with rounded-off edges.†

In order of importance this feature should be spoken of first, since it is that which gives the primary character to the scale; but as due prominence has been given to it in a former paper, it will not be necessary to dwell at much length upon it here, particularly as a more extended examination of the scales of insects has tended to strengthen me in the views I there expressed, rather than to alter them.‡

To illustrate the three features which I have endeavoured to enumerate, as puzzling us in our examinations of insect-scales under high powers, I have selected an accidental experiment, which I thought so curious, as to make a drawing of on the spot. I had been mounting some slides of the scales of *Petrobius maritimus*, and noticed in some of the slides that the cement ran in under the glass cover. Several of the scales confined a bubble of air between them and the cover for some minutes after they were surrounded by the cement; and the appearances which this gave until the air-bubble became disentangled from the scale, and floated away as a globe, were carefully watched. One drawing gives the aspect of the surface of the scale accurately focussed, showing the corrugations very distinctly in the *bubble portion*, and the granular bodies are roughly indicated in that portion immersed in the cement, to which I have referred as "Pigment." The other, representing the result of a little deeper focussing, gave a rather better image of the aforesaid pigment granules, while the beaded appearances which I look upon as spurious put in a strong claim to notice in the bubble-position. Strange to say too, these were in alternate longitudinal rows of greenish-yellow and blue, a phenomenon, however, with which my object-glass (Merz $\frac{1}{18}$) may have to do, since it always gives a good deal of colour.

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* It may be that these pigment granules often exist in the membranes of scales in an almost colourless condition. I believe they are almost universally present in the membranes of highly-organized scales, such as those of the Lepidoptera; and the only scales where I have been unable to discover them myself are those of the Thysanura, making exception, however, to the scale of Petrobius, which presents much similarity to the scales of many Lepidopterous insects, and in which they exist beyond a doubt. It does not follow, however, that because I have not been able to convince myself of their presence in Lepidocyrtus, Seira, and Templetonia, that therefore they are absent in those genera.

† Not always *rounded off*, however; sometimes they form rectangular divisions in the scale. (See Dr. Maddox's note appended to this paper. He deals with this portion of the subject more thoroughly than I do.)

‡ It will be recollected that, in that paper, I made exception for the existence of pigment and iridescence, which needed a more complete examination than I could then make, ere I could comment on Dr. Pigott's paper, read the same evening as mine, and with the contents of which I was previously quite ignorant.

To exemplify what I take to be hemispherical embossings of the membrane, one of the modifications of corrugations which is exceedingly common, I bring forward the scale of the inland Petrobius found by Dr. Gray at Dolgelly this summer. The scale is much like the finer scales from its marine relative, but perhaps shows the embossings a little better than these do generally. I do not think the insect has been found before, and its scale is certainly new to microscopists.*

This variety of corrugation, which may be likened to embossing, is the nearest approach that I can find to *real* beads in scales, and as it is exceedingly common it can be easily examined. The dots are wonderfully distinct on the dark scales of *Urania Leilus*† and *Euplœa Midamus* and many others, and if the slide is mounted so that a drop of water can be introduced under the cover while the scale is in view, much assistance is rendered to the investigator.

I would here take the opportunity to allude to the glimpses of the mode of development or plan of structure which is displayed in immature scales. The suggestion was made to me some time ago by Mr. Lowne, but the difficulty of its investigation almost put a stop to further researches on my part, though its force was apparent to me at once. Where then shall we look for materials to study? I think we have on any slide of Podura scales the very materials to hand. Everybody knows that slides of Podura scales contain vast numbers that are too small and too difficult of resolution to examine with satisfaction, and so we all move these out of the field in order to bring the largest only under view, and we look at these time after time, and express our puzzled opinions as to the structure in terms which need only to be compared with each other to show there is a great want of harmony in them.

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* The scales of Macrotoma (Poduridæ) are also excellent examples.

† See Figure on Plate LXIX.

I believe the Thysanura give the most remarkable examples, perhaps the only ones, where the change of skin takes place at a time when the scales are in all stages of development. From that moment, I believe, the growth of the scales is arrested; they dry up, and, by forming a somewhat thick layer of elastic corrugated plates over the delicate skin of their owner, provide a protective covering admirably adapted to the wants of the fragile creature they adorn. Their slender attachment to the skin is an additional evidence of Creative design; for were they to remain attached in spite of all the concussions the Poduræ are subject to in their active leaps, the tiny creatures must suffer endless contusions.

Now the smallest scales, as I see them, appear to be mere filmy sacs; those a little larger give some evidence that the membranes collapsed in drying, but they are so delicate that the wrinklings which such a process would give rise to are very indistinct.* Larger scales display the feature more and more strongly, and in the largest of all we see those wrinklings have acquired importance enough to occupy the patient attention and study of hundreds of clever observers. So much for the Poduridæ.

In the Lepismidæ (Lepisma and Petrobius) we get yet more information. The organization of these insects being of a higher type, it is reasonable to expect to find a higher and more complicated development in their scales; and this I think we do find. As I see them, and in my former paper I have dwelt on this point in detail, they form the link between the lower forms of scales and those possessed by the Lepidoptera, Diptera, &c.

The unintended length of this communication precludes my touching upon *Iridescence*, a subject which may at some future time be attempted, at least so far as bringing forward some curious effects in iridescent scales. I will therefore sum up the principal notions that the above experiments have led me to form, previous to making a few comments on Dr. Pigott's paper.

1. That the principal structural feature in insect scales is corrugated membranes—a plan ensuring the maximum amount of strength and elasticity with the minimum of weight.
2. That there are a few scales having one surface hackled.
3. That the ornamental requirements of scales are fulfilled either by iridescence or the possession of pigment granules in or upon the membranes.
4. That the beaded appearances seen in scales are due to the following causes, either singly or collectively:—
    - *a.* Corrugations, taking the form of hemispherical embossings.
    - *b.* Pigments.
    - *c.* Shadows of projections, or folds in the membranes, either within or beyond the focus of the object-glass.

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* Of course there is a predisposition for wrinklings of a definite pattern.

Though I do not feel satisfied that Dr. Pigott is correct in speaking of the basic membrane of a Podura scale, above and below which he says beads lie in chains, I yet think what he sees has some foundation; and it is our business, in the absence of experience with his aplanatic searcher, to ascertain if what he shows us bears any analogy to, or is conflicting with, ascertained structure in other scales. If the latter, no amount of optical knowledge on his part would convince me he is right; and, on the other hand, if analogous appearances have been seen in similar structures before, I shall be prepared to accept his views, subject to correction.

Now, it is clear to my mind that Dr. Pigott's discovery as regards the scale of *Lepidocyrtus curvicollis* admits of one or other of two interpretations:—either the appearances he sees are caused by pigment granules in the membranes, identical with those seen in the scales of insects generally, but which have never yet been asserted, so far as I know, to exist in Podura scales, or he sees "ghost-beads," which are always the result of some surface inequality, such as minute corrugations of the membrane or membranes, the existence of which minute corrugations in the scale of *Curvicollis* has likewise never yet been suspected. And, whichever interpretation be finally accepted, I for one admit that a step is gained, and Dr. Pigott can claim the credit of being the first to notice it.

This result places me in a position to view with favour the method by which he has obtained the appearance. Another consideration is, that it is highly improbable that the lens Dr. Pigott has used in the investigation is the only exponent of the truth, such as it is. (I think he admitted something to this effect at the October meeting.) Can I then test the value of this method of examination by means of the apparatus I have by me?

I have endeavoured to obtain upon *L. curvicollis* the same results as he does, and on two occasions was approximately successful. My process was as follows. The lamp was on my left, and about ten inches from the microscope which had the achromatic condenser (Powell and Lealand) on, and the smallest aperture of the diaphragm accurately centered. The mirror was adjusted a little to the right of the axis of the microscope, so that only enough light to illuminate without flare passed up through the lenses, and the illuminating beam consisted of rays rendered parallel. It seemed to be indispensable that the few large scales (for it was only a few in which I thought I saw it) should lie in exactly a horizontal direction, with the shafts pointing to the left of the field of view. The scale of *Degeeria domestica* (or *Seira domestica*, for Sir J. Lubbock has recently given the insect a new name) showed the same, or very nearly the same, still more strongly. I went through the whole of the Podura and Lepisma scales and saw beads abundant everywhere. Although I had seen the most of these beads before, it was plain to me that by accident I was getting results closely resembling Dr. Pigott's—and this too without the adventitious aid of an aplanatic searcher. I tried to obtain a witness in the person of a lady, as Dr. Pigott did; but, I am sorry to say, that until I told her to look for "beads," her testimony was not valuable, except as showing either how strong a part the imagination plays in spite of ourselves, or else that I needed an aplanatic searcher to make all clear.

As regards this *tour de force*, I have by dint of a good deal of inquiry and observation obtained a notion which is more or less correct. My notion is that it simply consists of a species of object-glass* of 1 inch or $1\frac{1}{2}$-inch focus (or thereabouts) placed between the eye-piece and observing objective. It is capable (within certain limits)† of being shifted to any distance which practice shows to give the best results. The larger of the two lenses forming the aplanatic searcher is directed towards the observing objective and the smaller towards the eye-piece. Thus explained, some observers may recognize it as a plan which has already been put in practice for amplifying the power in their own microscopes, but discarded as unsatisfactory. Moreover, I have, by the kindness of Mr. Curteis, been favoured with the loan of one of these instruments constructed by him on Dr. Pigott's principles. The "searcher" was a $1\frac{1}{2}$-inch objective originally constructed for photographic purposes by Dallmeyer.

The results were much better in my hands than I anticipated, and, so long as comparisons (which the proverb says "are odious") were not made, they might be called *good:* but directly this test of their goodness was tried, the verdict went against the "searcher." While there was great gain in amplification there was a palpable loss in definition: less certainly than I was prepared to expect, but still a palpable *loss*. The objectives used in this examination were,

$\frac{1}{5}$ Smith and Beck,
$\frac{1}{4}$ Powell and Lealand,
$\frac{1}{8}$ Andrew Ross,
$\frac{1}{12}$ Powell and Lealand (dry),
$\frac{1}{18}$ Merz (immersion),

and each and all of them performed better without than with it, so far as giving a brilliant and well-defined image. The objects chosen were *Pleurosigma Formosum* (dry), and the whole series of Thysanura scales. Mr. Fitzgerald assisted me in the examination, and I believe came to the same conclusion as myself.

On a second occasion I used an ordinary $1\frac{1}{2}$-inch objective by Ross as a "searcher,"‡ and obtained such good results that I was nearly altering my judgment upon it. But, remembering that I could fit up my second microscope with equal power, exactly similar illumination and duplicate objects, and place both microscopes so that I could look first into one, and then into the other, without getting off my chair, I determined to put the plan in practice; and accordingly did so. As before, comparisons were odious. The image was much clearer and better defined in the microscope where the amplification was obtained in the objective only, than in the other. I think on this latter occasion the definition was at least as good as the best I ever saw under Dr. Pigott's own microscope; and certainly it was greatly superior to that given in his instrument at the meeting of the Royal Microscopical Society in October last. (I mention this to show my conviction that there is no *material* difference between the searcher he uses, and the one [*] lent to me.)

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* Does it differ from an ordinary object-glass more than object-glasses of the same focus by different makers differ from each other?

† The contrivance by which this is effected is ingenious.

‡ Subsequently I tried a $\frac{2}{3}$-inch by Smith and Beck with good results.

The objects which he has hitherto exhibited are not by any means crucial tests. They are *P. Formosum* (dry), scale of *Degeeria domestica*, and scale of Macrotoma (named on Dr. Pigott's slide *Podura plumbea* [†]). I have always been under the impression that I had seen them better without any searcher at all, in the microscopes of Dr. Gray, Mr. Fitz Gerald, and Mr. Delferrier, and also in my own; and there is evidence in the celebrated American photographs of the Test-Podura scale (Lepidocyrtus) that the so-called beads have not been wholly obscured from view.

I know that the lens I have used is not the exact combination which Dr. Pigott has had made, but I get precisely the same abundance of beads as he describes; and the same abundant display of brilliant blue and yellow, green and ruby effects which he dilates upon are associated with all its best performances. Moreover, there is the same subtilty of adjustment of which he speaks—a difficulty which Dr. Maddox (I believe) finds nearly unsurmountable: a point upon which I also can speak feelingly, since I damaged my "fifth" objective (by Smith and Beck) in the endeavour to obtain it accurately. Then there is the occasional excellent definition to counterbalance the unbelief which so lavish a display of colour in objects, usually transparent, infuses into one's mind.[‡]

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[*] I retain the term "aplanatic searcher" in speaking of this contrivance, for the obvious reason that Dr. Pigott has used it, and until Dr. Pigott corrects me I must still believe that it is properly so called.

[†] The genus Podura is non-scale-bearing, and is represented by the active black insect which skips over the surface of the Hampstead Ponds, looking at a distance like a quantity of soot on the surface of the water.

[‡] Dr. Maddox writes to me that he did not get on well with the aplanatic searcher which Dr. Pigott was good enough to lend him. He says its value seemed to be chiefly in the facility with which many structures gave complementary tints very defined, thus facilitating the study of tissues of different thickness. But he further says the results were better than those obtained by amplification with an ordinary objective of low power at the end of the draw-tube,—a plan he has tried in company with the late R. Beck; and does not seem to value results of this kind, unless daylight and artificial illumination tell the same tale. He also makes some valuable observations upon chromatic phenomena, which I have asked him to put in a convenient form, as they are likely, as mere extracts, to lose much of their force.

Finally, in spite of the disadvantages and difficulties which seem so far to attend this mode of examination of objects—difficulties and disadvantages that in the light of more knowledge and more experience may be dissipated, I do occasionally get glimpses which surprise me. I will not venture to pronounce an opinion upon them, but merely lay the whole matter before you as a subject worthy of careful exploration, and certain to yield most interesting results. I notice that, whatever other adjustments are necessary as regards the correcting collar of the observing objective or the aplanatic searcher itself, it seems to be imperative that a *distance of not less than seven inches* shall be kept between the eye-piece and the searcher, at risk of cutting the margin of the field of view.

It was my intention to have exhibited an experiment with it, but the accident to my ⅕-inch objective has warned me not to attempt it under circumstances so unfavourable to perfect success as the present occasion, but I hope to do so ere long. I append an extract from one of Dr. Maddox's letters to me on the structure of scales, and I look forward to further observations by him in elucidation of many difficult portions of the subject, especially the chromatic effects:—

Extract of Letter from Dr. Maddox.

"My object was to try amongst the readily procurable and ordinary scales the effect of various chemical and destructive agents, and I fancy by these means we may make out much of their structure aided by the microscope, though with it alone I fear we shall be constantly verging on the dubious. Whatever the surfaces, there appears a framework amongst them which is connected to the inner surfaces, and the nearer the form of it corresponds to small squares, the nearer the approach to beading, and the coarser at the corners or junctions the more resemblance to spherical embossings.

"These spaces seem to be filled with a greasy matter partly soluble by boiling in liq. amm. fort., then treating with chloroform at a boiling point—performing these operations on the slides, and allowing the soluble matter to be washed away by the solvent last used. Then staining the scale with *aniline* or other suitable material and mounting in a dry state, also in a dense liquid saturated solution of acetate potassæ, &c.—or boiling in liq. potassæ, washing with distilled water, then with ether and chloroform—and drying by heat—in fact varying these agents; also using turpentine to boil these scales in, and then permitting some thin coloured resin in solution in turpentine to flow over them and dry by heat. Numerous methods were tried and under high powers, advantageously as regards structure."

III.—*On an Optical Illusion Slide: Cracks in Silica Films.*

By Henry J. Slack, F.G.S., Sec. R.M.S.

(*Read before the* Royal Microscopical Society, *Nov.* 9, 1870.)

Although there may be much reason in the complaint of microscopists that existing objectives leave them without the means of distinguishing many characteristics of minute structure, such as ultimate organic tissues, cell walls, &c., it is certain that the progress of knowledge is more often impeded by difficulties of interpreting what the best instruments can show. The long-continued controversy whether the markings of diatoms consisted in elevations or depressions, the discussions concerning the scales of Lepidoptera and Thysanura, together with many others that will occur to the practical student, are illustrations of this fact, and we can only expect to arrive at greater success in discriminating between appearances optically true under certain conditions, and veritable facts of form and texture, by becoming familiar with the circumstances under which illusions arise. As a small contribution to this end, the attention of microscopists is called to "An Optical Illusion Slide," which may be thus prepared. Place a drop of an aqueous solution of colloid silica, obtained by dialysis,* on a glass slide, and evaporate it, either quickly over a lamp, or slowly in a place free from dust. By this means a film of transparent silica is deposited upon the glass, fissured in curious and complicated patterns by numerous cracks. These cracks when highly magnified present a variety of forms, something like a veined leaf, or the patterns on a complicated city map, with long lines of straight and curved streets, squares, ovals, circuses, &c., and in certain places all sorts of angular and rounded outlines. In general the cracks take place without materially altering the level of the surface, but occasionally an edge will be found turned up.

If a slide thus prepared be examined in diffused daylight, its real character is not difficult to discern, especially if attention is paid to the widest of the fissures and the largest of the isolated silica plates. By lamp-light, however, all the finer cracks, viewed as transparent objects with a power of from 200 or 300 and upwards, look like elevations, and that focussing which exhibits them in the strongest relief would be supposed to be the most correct, from the superior distinctness of its outlines.

By the employment of a central stop of an achromatic condenser, the illusion becomes more striking. If a large angle of aperture is used and the field flooded with light, prismatic colours appear abundantly, one edge of the crack being blue and the opposite one red, and in many places rows of narrow coloured bands like Newton's rings will be noticed, especially if the power is high enough—say 600 to 800 and upwards. Unilateral light does not clear up the question, an eye being still best satisfied with the focussing that brings the lines of pseudo-elevation into the greatest sharpness and relief.

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* Silicate of soda or potash is dissolved in distilled water, enough hydrochloric acid carefully added to separate the alkali from the silica. The liquid is then placed in a dialysing drum, and the crystalline salt escapes, leaving the silica dissolved in the water that remains.

With dark-ground illumination a very beautiful effect is produced; the edges of the cracked silica film being brilliantly luminous, while the uncracked portion of the film, and the interspaces between the luminous edges of the cracks are dark. The larger portions of the fractured silica, and those most detached, show the true nature of the object with this illumination; but the same can scarcely be said of the finer cracks, which might easily be taken for small tubular vessels anastomosing in all directions.

In these experiments, the eye is most readily deceived by a high power. For example, let the slide be viewed with a $\frac{1}{2}$-inch and an A eye-piece, with transparent illumination; the true nature of many cracks will be seen; then substitute a C or D eye-piece, and the eye becomes most doubtful of that focussing which is nearest the truth; the aspect of bright clear ridges being most satisfying and agreeable. The reason of this is evident. When the *surfaces* of the silica films are focussed, there is nothing particular to see, as they are smooth, clean, and exceedingly transparent, while the cracks look dingy and imperfectly defined. A little lower focussing does not seem wrong, because there is nothing on the silica surface to become indistinct, while the cracks appear in fine relief, rounded and often slightly tinged with colour from refractions, or reflexions, or both combined.

A $\frac{1}{2}$-inch or $\frac{1}{10}$-inch Lieberkuhn reveals slight inequalities in the surface of the split films, and when they are illuminated with Messrs. Powell and Lealand's modification of Professor Smith's illuminator for opaque objects with high powers, many fragments of the film exhibit prismatic bands, and some show complete series of Newton's rings. It is necessary to use the smallest of the diaphragm stops with this apparatus, or the field will be flooded with misty light. The larger fragments give the best colours, which seem to arise from the tension of the silica in drying, leaving minute air-spaces of varying thicknesses between the glass side and the films. The coloured bands vary in shape from approximate circles and ovals to other curves of various characters. The circular form is best shown where the film has solidified round a central boss.

Portions of the films sometimes appear of the same dark-grey tint of the ground, while others are luminous. This appears to result from the obscure parts being in such complete contact with the glass, and with upper surfaces so completely parallel to the glass surface as to produce the same effect upon the light which the glass surface does.

IV.—*Object-glasses and their Definition.*

By F. H. Wenham, Vice-President R.M.S.

Our thanks are due to Dr. Pigott for his laudable attempts to advance the microscope object-glass, but as he appears to stand forward as the pioneer of a new era in their construction, ignoring all glasses made before the publication of his first essay as "old-fashioned," such dictatorship naturally challenges inquiry as to the merits of his investigations, and whether they have in any way contributed to the end in view.

His papers published in the July and September Nos. of the 'Monthly Microscopical Journal' for 1870, only tend to mystify the simple action of the immersion lens, for he therein endeavours to show that there are some new and hitherto unnoticed optical conditions peculiar to the immersion front, and brings forward the remarkable error that, if various fluid media, such as water, turpentine, &c., are introduced between the cover and front lens, a larger aperture or greater angular pencil of rays is transmitted from a radiant point or object, *when mounted in balsam*, but in fact the angle must necessarily be limited on such objects.

In the year 1854 Professor Robinson and myself * demonstrated simultaneously that the angle of illumination, and also the aperture of the object-glass, were reduced on objects mounted in Canada balsam: Professor Robinson assigned the limit as 81°, I stated it at 82°. This slight difference refers to the refractive index of the glass cover, which was not taken as the same in each case. The pencil must of necessity be confined within the angle of total reflexion; this may vary from 40° to 41° 30″, according to the density of the crown-glass cover. This so far is an established optical fact. Dr. Pigott's inference is, that by the introduction of fluid films of various refraction, between the front lens and cover, more or less of the entire aperture is again recovered and utilized, as the total reflexion of the upper surface of the cover is then prevented, or is extended definitely according to the refractive index of the interposed medium employed, and that a greater angle is transmitted in consequence. This assumption is taken quite regardless of the dimensions and construction of the component lenses of an objective, and their capability of admitting rays in excess of a certain angle. The paper is prefaced by the following remark,—"One might almost venture to declare that perhaps too much exclusive attention has been paid to the objectives, to the neglect of the pencils radiating from the object . . . the primary behaviour of the *tiny spray of rays* is of the last importance to the final definition."

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* 'Quarterly Journal of Microscopical Science,' July, 1854, p. 212; ibid., Jan., 1855, p. 165.

This is ominous of the series of errors which the author afterwards brings forward. Anyone that has worked practically at the subject will admit that the focal point of an object-glass of large aperture is confined within such a very minute limit as to distance, that it is impossible to assume its position for the purpose of tracing the course of a ray through the entire combination. In fact, the work must be commenced from the posterior or conjugate focus, which, though a variable distance according to the power of the eye-piece or its position, is in some cases so far back that rays only slightly convergent or nearly parallel may be commenced with, and the result will approach near to accuracy in the focus, or final convergence beyond the front.

The same optical law that limits the aperture of any object-glass to near 82° in a balsam-mounted object also determines the angle *in the lens* at which the rays diverge *after* being refracted from the plane surface of the front. *This can never exceed* 82° in a dry objective; nor can it be greater on the immersion system, where an interchange of front adapts it to both conditions, as the very correction which necessitates the form of the back lenses and their diameters will not transmit a greater pencil; and therefore if the front is immersed in balsam for the purpose of viewing an object placed therein, this angle of 82° or less, as the case may be, instead of converging at 170° as from the dry lens, is continued right to the object, supposing the refractive index of the front and balsam to be the same, which they are nearly.

In Dr. Pigott's diagrams, p. 134, Plate LX., of the September No. of this Journal, Fig. 2 is a form of achromatic that is now never employed as a front. The focal point in each example is taken in quite an impossible position, for the rays there shown arrive at the convex back of the lens nearly as a radius, or from its centre of curvature, so that at this surface there would be scarcely any refraction, the rays passing through nearly straight, most of them never reaching the succeeding lens at all, and those that finally emerge, instead of forming a conjugate focus behind, would be *divergent;* and yet after quoting my objections he still complacently refers to his Plate LX. as the standard for illustrating "the aberrations of the two systems."[*]

The annexed diagram, Fig. 1, $28\frac{1}{2}$[†] times the size of the original, shows the marginal rays of mean refraction projected in the exact position that they occupy through the $\frac{1}{8}$th described in my paper "On the Construction of Object-glasses."[‡] The focal point from the front lens is ·0126, or near $\frac{1}{80}$th of an inch, the extreme aperture is 130°, and the rays will not enter beyond this limit, therefore the ray after refraction by the flat front must occupy a fixed position which cannot be exceeded, and so on during the whole course of the ray through the entire combination, till it converges to the long conjugate focus at the back. These lines being unalterable, it must be evident to the merest tyro in optical science that if the front lens is immersed in Canada balsam, but little or no refraction can take place at the flat surface, and the rays from this, instead of meeting at 130°, will proceed straight on, as shown by the dotted lines, at an angle of about 74°, showing the loss of aperture in balsam. I am here assuming for the sake of simplicity that there is no thin glass cover intervening in the mass of balsam between the object and lens, as its presence would not materially influence the result.

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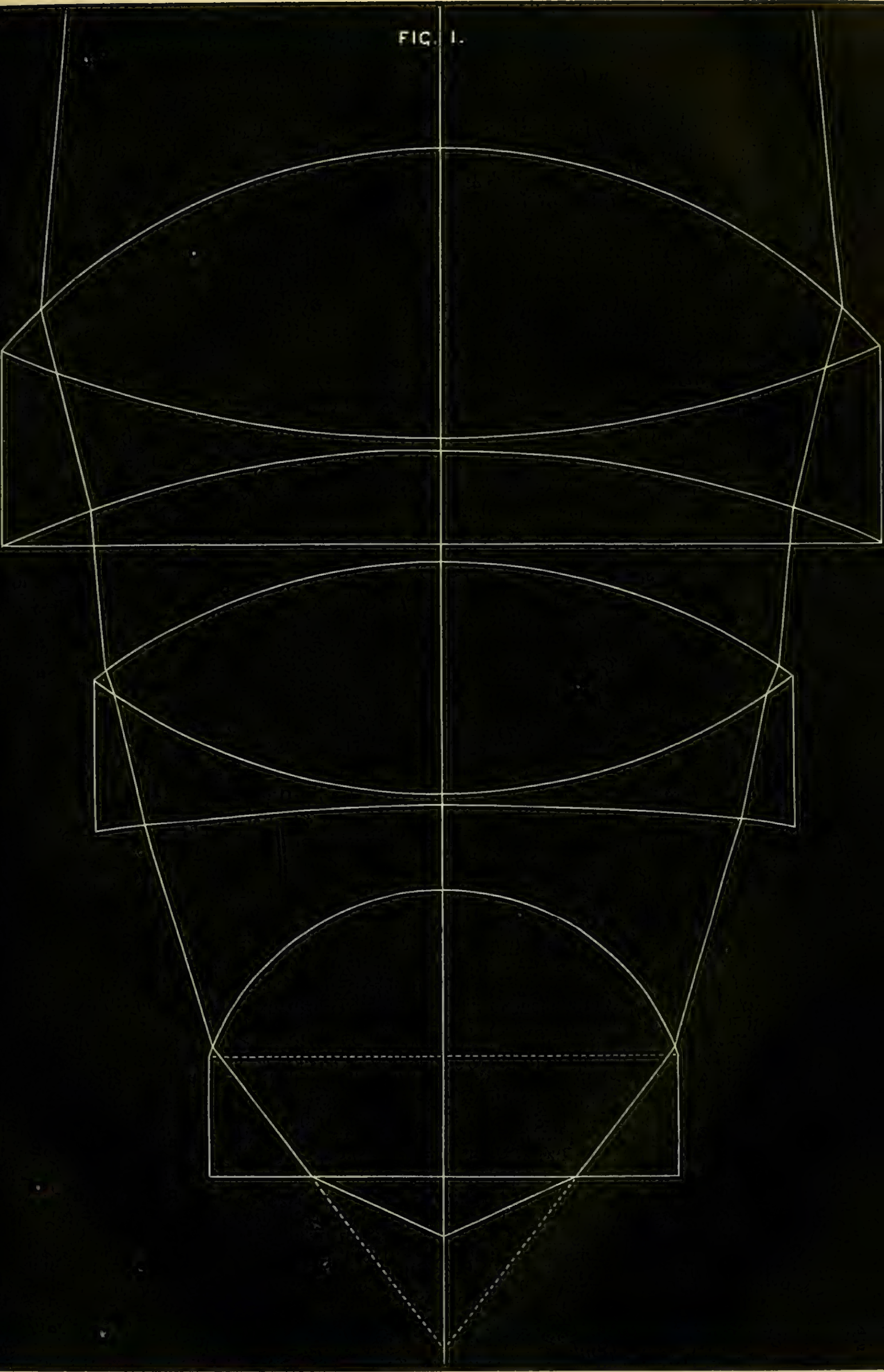
[*] See p. 254, Nov., 1870.

[†] The original diagram was much larger, but was required to be reduced to bring it within compass for these pages.

[‡] 'Microscopical Journal,' Feb. 1869, p. 112.

I may, now that the diagram is under consideration, make a few remarks concerning this $\frac{1}{8}$th, as its performance is known to many. Comparisons were often made, and it held its own for some years. This is not adduced for the sake of exalting it as a model of perfection, but for the discussion of possible defects, and the direction in which improvements might be anticipated. Commencing with the front lens. The simplicity of this, and also the determination of the radius by the intended power of the lens, leaves but little scope for alteration. The correction for thickness is obtained by trial, as I have explained in my paper. The diameter is limited by the proposed aperture. For 130° rather more than this segment of the circle must be allowed. For 170° we approach near to a hemisphere. We now come to the middle; it will not do to make this of too long a focus, or we cannot command our aperture, therefore the curves are deep and the lens thick. A short radius in the back emergent surface is not material, for the bending of the rays is here gradual; but in all achromatics it is important to get the contact surfaces of as long a radius as possible, hence the value of the triple back. Now in the middle here delineated these surfaces are deep, so far, that were it not for the Canada-balsam film that cements them together, which allows the transmission of the outer rays, these would be lost from total reflexion from the concave surface of the flint, as shown by the dotted line. As such abrupt courses are prejudicial both for the correction of chromatic and spherical aberration, the question is, how can this be remedied? 1st, by making the lower surface of the concave deeper. This can only be done to a limited extent, or we have an excess of outward coma that is afterwards difficult to cure; indeed if the quality of the glass were suitable I would prefer to make this surface flat. Another way would be to make the middle a triple. Andrew Ross employed this, but abandoned it eventually, probably on account of its complexity. If the first and most abrupt bending of the rays were sustained by two

FIG. 1.



single lenses in front, they would emerge more nearly parallel, and pass through the middle at a more favourable angle, which could therefore be made with a longer focus and radius. But the best remedy would be an improved quality of glass. The flint with a less refractive and higher dispersive power, and the crown *vice versâ*, I believe it possible that such a glass could be made. Of the triple back little need be said, as the bending of the rays is here easy: before this was introduced the old form had the defects to which I now call attention in the present middle, and the marginal rays were imperfect and limited the aperture.

Speaking of recent improvements, Dr. Pigott alludes to an object-glass made on the Lister principle as being "old-fashioned," from which I infer that he considers them quite obsolete; but as long as the adjustment between the lenses exists for the purpose of obtaining a correct aplanatic focus, so must all object-glasses remain on the Lister principle, and it is the absolute correction of this focus for chromatic and spherical errors upon which their perfection must depend.*

A great advance has been recently effected in the highest powers, *which are now made with a single front;* this after remaining for years unnoticed has triumphed at last. This formula also affords peculiar facilities for the addition of an immersion front, as I shall presently show, and to which its success is mainly due.

I will now make a few remarks concerning the Podura. In the November number of the Journal, Dr. Pigott has quoted and italicized some sentences from my communications, and placed them so as to make it appear that I contradict myself, and have been undecided in the structure. These sentences, as my papers will show, referred to the difficulty of proving the spines to be projections by some other means than mere sight, and obtaining more mechanical evidence of their form. I said that in a doubled-up scale "the markings ply round the sharp bend so closely that the keenest eye cannot detect any appreciable rib or projection;" but this assertion I must withdraw, as Mr. Beck informs me that in the coarse specimens, of which he has a variety, the ribs can be seen plainly projecting at the bend; and his recent communication to the Royal Microscopical Society concerning the moisture experiment, furnishes another proof that the markings are longitudinal ribs projecting mostly on one side of the scale, and are not caused by similar rows of beads crossing obliquely on opposite sides as Dr. Pigott states. Whether these "beads" in the form stated are fallacious or not must be left for the present; there is probably some analogy between them and those on the ribs of butterfly scales; but if an object-glass fails to discover any indications of them, the fault may be in the scale—some of which will show nothing of the kind, for there is a singular difficulty in obtaining boldly-marked scales in some localities. Dr. Pigott says that in my last communication I have "sketched the Podura markings something like an Irishman's shillalah." Not being familiar with the look of that atrocious weapon, I do not feel the force of the simile; but I have a very beautiful and exquisitely distinct photograph of the Podura, kindly sent to me by Colonel Dr. Woodward, one edge of which has rounded away from the cover to which it adheres, bringing the markings gradually into profile in the form of my sketch. But Dr. Pigott repudiates photography, saying that it fails to reveal what the eye alone can detect, and that the photographer has never yet displayed the *Amphipleura pellucida*—simply, I presume, because it has never yet been tried. When markings have been very difficult to show by ordinary illumination, I have frequently been surprised at the wonderful facility with which they are displayed in a solar image; in fact, Dr. Woodward's photographs of Nobert's 19th band magnified 2800 diameters (a series of which he has also sent to me) tend to prove this after the long discussion as to whether they have been seen or not.

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* I may here offer a passing tribute to the memory of the late Joseph Jackson Lister, to whose researches the present advance of the microscope object-glass is entirely due, for in the perfection of aplanatic foci, caused by difference of correction, and corresponding distance between the lenses, he enunciated a fact as definite as Newton's law of gravity. Some inclination has been shown to question this; but it must be remembered that in order to carry out his ideas, he had to avail himself of the skilled workmanship of the then three rival makers, who, doubtless individually, effected many improvements in detail in the particular power that they engaged to construct, and each justly considered that he had some right to a monopoly in the special objective that his workmanship had made perfect. This rendered Mr. Lister's position rather a delicate one, and his remarkably amiable and kindly disposition forbade him from coming forward to the injury of others, and his retiring manner shrunk from anything approaching to public display; hence he has been unjustly suspected of reserve. I visited him at his residence at Upton on two occasions, and on each spent several interesting hours in going through all his plans and experiments, which were brought forward in the most candid and confiding manner. Every proposed construction had been worked out, with all the rays accurately traced through, in large and neatly-constructed diagrams, before they were carried out practically; and I should be glad to see these very comprehensive plans in the archives of the Royal Microscopical Society, as I know for a fact that $\frac{4}{10}$ths, $\frac{1}{4}$ths, and $\frac{1}{8}$ths, were made therefrom, with no other alteration than a slight variation in the quality of the glass required.

I must protest against the positive manner in which Dr. Pigott still totally condemns the mercury globule as a test for the construction of object-glasses. This self-confidence may be attributed to non-practical acquaintance with the use of it. That it is employed solely for the purpose of obtaining a distinct image is quite a mistake. The greatest value of its indications are when it is considerably within or without the focus. Commencing with the back triple, this is first corrected alone on the globule, and the coma observed both within and without the approximate focus, gives the indication whether the lenses are right, or what alterations will be needed to make them on. No optician thinks of the possibility of a distinct image with the excessive aberrations both chromatic and spherical *purposely* introduced in the back combinations, to be afterwards neutralized or corrected by the counter-aberrations of the front. In this way the globule is used throughout the series; I know of no substitute; and so reliable and comprehensible are its indications to the worker of lenses, that I have no hesitation in repeating this, my former assertion, commented upon by Dr. Pigott, "that without this test it would be impossible to construct perfect objectives." That the separation of points of light on the globule coming from two sources placed at a given interval is a test, no one will question. A bad glass will blur them together, and a good one will separate them more or less distinctly. Andrew Ross employed this method of separating known intervals, in his experiments on the defining power due to aperture. Dr. Goring used the reflected image of the bars of a window, as explained in "Micrographia;" and the globule-reflected image of a Venetian blind, adjusted so as to regulate the width of the dark and light intervals, also makes an excellent test; or the wire gauze and perforated zinc employed by Mr. Lister.

As again quoted by Dr. Pigott, I have stated in reference to the immersion lens that "not either the water or glass cover has introduced a single new element of correction," meaning that these are combined identically as part of the front lens. This assertion may have appeared dogmatical, but was based upon practice. My experiments in the construction of object-glasses have been conducted more with the view of originating new combinations than repeating the same thing. The single fronts were made very thin to begin with, and the measure for the necessary thickness for correction was arrived at by means of different thicknesses of glass stuck on the front with Canada balsam, but more frequently by *water;* the dimension thus obtained was employed for finally making another front of the correct thickness. The object-glasses were strictly immersion lenses at this stage of their existence. I did not then think it worth while to retain a special immersion object-glass (*i. e.* one with a *thinner front*), particularly as several of the covers of my test object were cracked and could not therefore be used with water; but as some further proof of the particulars and dimensions are now needed, I have recently removed the front of a $\frac{1}{12}$th and substituted a thinner one of the same curvature to be used as an immersion. Fig. 2 is the dry lens fifty times the size of the original; the rays are taken through from beyond the glass cover, with the object-glass corrected for this thickness, which combines as part of the lens. Fig. 3 is the immersion front, wherein the lens, water, and cover form the unity, also corrected on the object for thickness. The aperture or angular pencil passing through the combination from a dry-mounted test is the same in both cases. In Fig. 2 the thickness of the lens

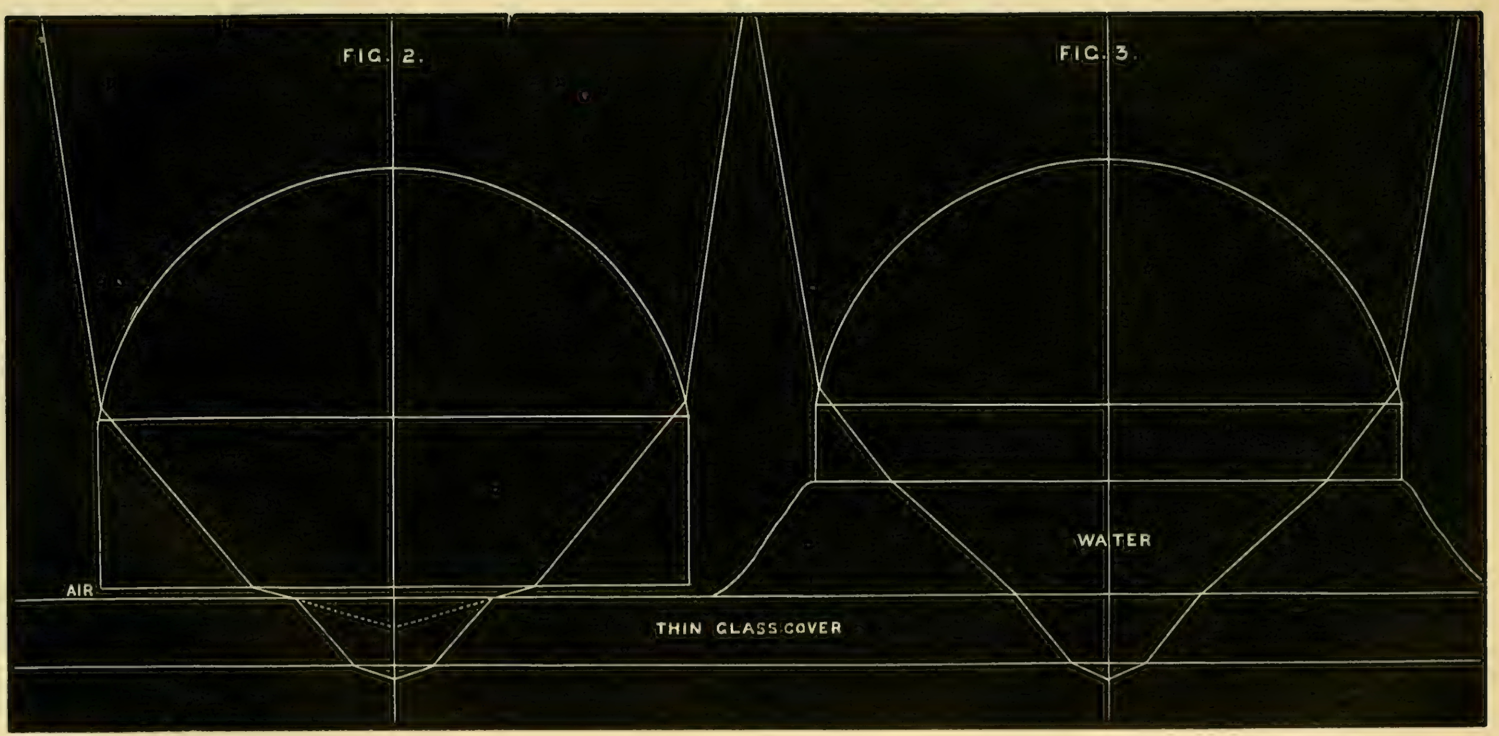


FIG. 2.

FIG. 3.

in fractions of an inch is ·044, radius ·0315, diameter ·062, glass cover ·007, distance of focus from front ·004, as shown at a point in the glass by the dotted lines. The thickness of the cover and lens together is ·051. In Fig. 3 the radius and diameter is the same as the dry lens—Thickness of lens ·034, ditto of water film ·012, ditto glass cover ·007, total combined thickness ·053; so that the corrected thickness of the water front exceeds that of the dry lens and cover by ·002, or $\frac{1}{500}$th part of an inch. This difference may be accounted for by the less refractive power of the water.

The effect of this immersion lens is to give greater clearness and brilliancy to the object, and render markings more distinct that were before scarcely visible with the dry lens. This is in fact attributable to the saving of light and comparative absence of refraction and reflexion from the top surface of cover and front of lens. But the great merit consists in the perfect correction that the adjustable thickness of the water stratum affords in compensating for every thickness of cover. Nor is the thickness of an immersion front a matter of particular nicety, for it can be made as thin as desirable; the water will occupy the place of the deficiency.

In the three diagrams I have accurately transferred all the dimensions, and it can at once be seen how a thickness of front ensures both correction and large angular aperture, by spreading out the cone of rays in its passage through the lens, after the first refraction, so that the marginal ones fall on the edge of the convex back, and then emerge in a direction suitable for entering the middle and posterior combinations, conveying the full angular pencil through the series.

I have not yet ascertained whether the "aplanatic searcher" has any influence in improving definition, as no description of its construction has yet appeared, and it exists more in name than in use.

V.—*On the Mounting of the Diatom-prism.*

By F. W. Griffin, Ph.D.

The unilateral system of illuminating transparent objects, introduced by the Rev. J. B. Reade, F.R.S., bids fair to be more and more generally employed as its important advantages become more widely known. But there are practical difficulties in fitting the diatom-prism to any large and complete stand, since to obtain the most oblique beam it has to be brought almost close under the slide, while elaborate traversing and rotating movements require a thickness of stage which keeps it too far off for its full effect. In the Ross model this thickness is certainly reduced to a minimum in the centre, and this suffices for all appliances illuminated from below. But the necessary depth of the outer ring prevents the side beam from falling on the prism, or the fingers from reaching its milled head, when it is brought sufficiently close up. As it is scarcely worth while to sacrifice the strength and durability of the stand to suit a single accessory, it is best to bring the diatom-prism *above the stage.* It is then completely unobstructed in its movements and illumination, and works much the same with *transparent* objects as Crouch's admirable parabolic reflector does with *opaque* ones. The fitting required consists of a stage-plate sliding in the same dovetails as the ordinary object-carrier (which has then to be removed), and carrying on four uprights, about two inches high, a thin plate cut out in the middle with spring-clips to hold the object-slide. This is of course carried, equally with the main stage, by all the traversing and rotating movements. The diatom-prism, mounted as usual, fits into the end of a tube carried by the sub-stage, and long enough to project through the aperture of the principal stage. The prism can thus, when desired, be racked-up close under the object with every facility for adjusting and lighting. The bottom plate is cut out on its lower side, so as to slide over this tube after fixing the prism in the upper end.

I may note for those who desire a cheaper and simpler, yet serviceable, arrangement, that the diatom-prism works very well with the excellent glass-stages of Messrs. Beck and Crouch, since these are thinner than stages traversing and rotating mechanically can well be made. As the rotation of these is satisfactory, and the sliding movement sufficiently smooth and delicate for use with $\frac{1}{8}$th inch, they are adequate for ordinary prism-work.

VI.—*On Pterodina valvata: a New Species.*

By C. T. Hudson, LL.D.

Plate LXXII.

Pterodina is rarely met with in the neighbourhood of Bristol; and I had not taken more than six specimens, and these all of *P. patina*, during the last sixteen years. Nothing can be more tantalizing than capturing a solitary *Pterodina.* It is so delicately transparent, that it is extremely difficult (indeed, I found it almost impracticable) to see it with the naked eye well enough to catch it with a pipette, in order to place it on the compressorium in a drop of water, whose diameter should not greatly exceed the creature's length; and yet with solitary specimens this is the only method by which a satisfactory investigation of its structure can be secured. Even when this is accomplished, *Pterodina* is of such evanescent thinness that I was unable to hold it between two plates of thin glass without driving the drop of water away from the rotifer. I made use of each of Ross' later compressoriums, and it is the first time that they have ever failed me; their admirable construction has hitherto enabled me to hold gently the minuter rotifers in the smallest possible quantity of water. The advantage of being able to bring two plates of glass together, within the distance of a rotifer's thickness, with a truly parallel motion, is this; that the drop of water in which the rotifer is placed may be made as small as the rotifer itself will bear without fear of the fluid's being drawn away from it altogether by the want of parallelism of the plates; for these, when inclined to one another at a very minute distance, act as a capillary tube, and whisk off the water in the direction towards which they incline.

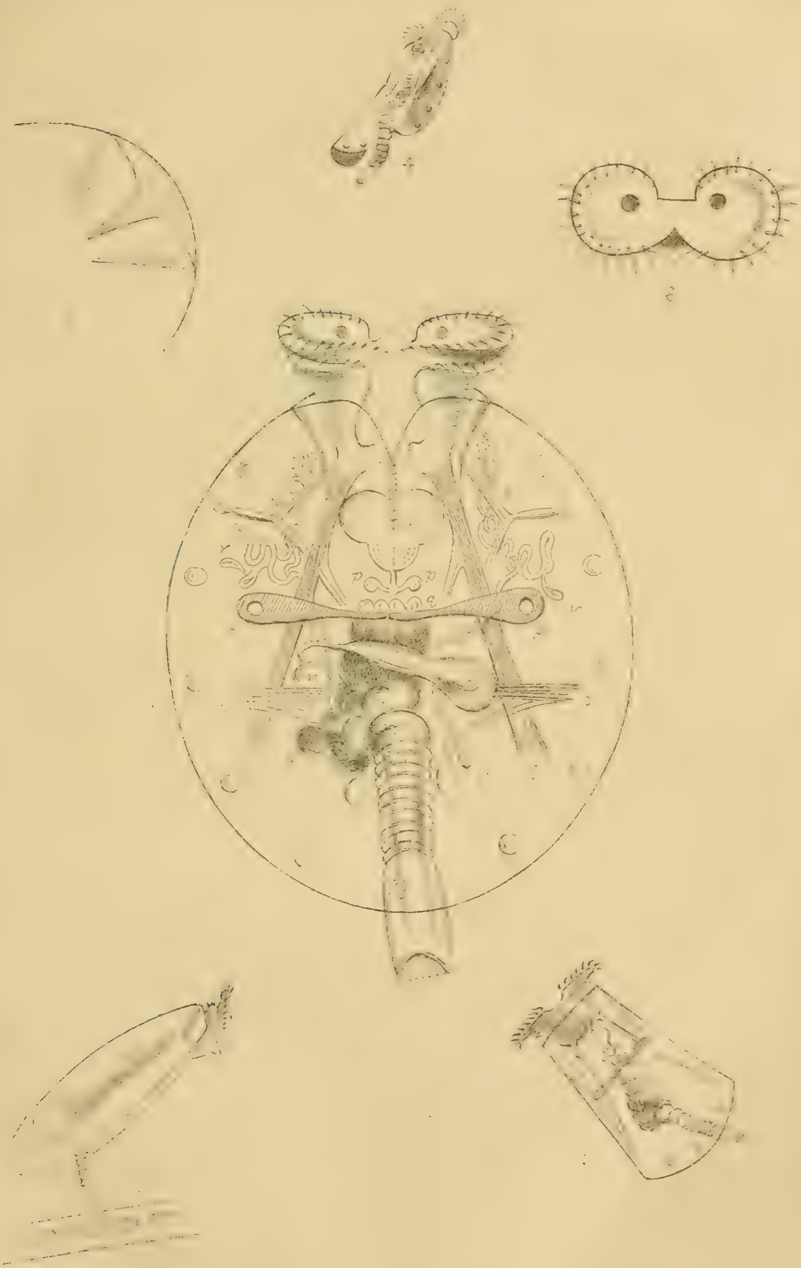
"But," it may be asked, "why place the rotifer in a drop scarcely bigger than itself?" This is done in order that the animal may be placed, once for all, without chance of escape, in nearly the exact centre of the compressorium, so that all the apparatus under the stage may be brought, if need be, to bear upon it, without risk of being impeded by the compressorium itself.

Moreover, when the rotifer has been so placed, if the observer gets tired, or the rotifer seems distressed by having been held for some time, the slightest slackening of the screw gives the creature a watery cage to swim in so small that it remains safely within the field of the objective.

If a single *Pterodina* happens to be placed with Algæ in the live cage, nothing can be more charming than to watch it under dark field illumination with a low power, but little can be learned of its structure. The thickness of the Algæ secures for it watery

EXPLANATION OF PLATE LXXII.

Fig. 1.—Under-surface of young female of *Pterodina valvata*.
m, mastax.
p, *p*, pear-shaped glands below mastax, and adhering to œsophagus.
q, similar glands, where the œsophagus enters the stomach.
g, *g*, gastric glands.
s, stomach.
o, ovary.
v, *v*, *v*, vibratile tags.
l, *l*, longitudinal striated muscles.
t, *t*, transverse muscles for folding lorica.
n, *n*, transverse muscles for closing the slit in the neck.
c, *c*, dilated ends of canals of water vascular system.
" 2.—Side view of *P. valvata*.
" 3.—Dorsal view of the same, with lorica folded.
" 4.—Side view of the same; lorica folded.
" 5.—Enlarged view of the under-surface of the front of the lorica—the animal withdrawn to show the flaps which are closed by the muscles *n*, *n*, Fig. 1.
" 6.—Trochal disk, viewed from above, and showing the two eyes.



Tuffen West sc.   W. West imp.

Pterodina Valvata.

depths into which it dives out of reach of any but low powers; and, moreover, it has a habit of forcing its delicately flattened disk between the interlacing filaments of the Algæ, and of thus hiding half its beauties from the observer.

In order to study the animal with any chance of success, it was obvious that I ought to find, not two or three specimens, but two or three hundred; as then, with a score or so in the same live box, I should be sure to find some one or other resting occasionally on or near the surface of the upper thin glass. This July I was fortunate enough to find a spot where *Pterodina* swarmed in countless thousands; the water was alive with them. It was an odd habitat. A large pond near Abbot's Leigh—the carp pond of an ancient monastery—is a favourite hunting-ground of mine; it lies in a natural hollow between fir plantations, and has been formed by damming up the course of a streamlet which runs through the hollow. Algæ grow thickly on the stones of the old weir, and Melicerta and Floscularia are generally to be found there with many free-swimming rotifers. I had pretty nearly filled all my bottles, when I thought I would take a dip in the square hole outside the pond in which the iron shaft of the weir gate works. It is not more than a foot square, and is as dark and unpromising a place as one could imagine; but here I found *Pterodina patina*, and a new species, swarming in countless numbers; and I have since obtained similar swarms of *Triarthra longiseta* from the same place.

Now I found *T. longiseta* before swarming in an open farm-yard pond, of which the water was so coloured with manure, that it was impossible to see through an inch thickness of it; and yet here it throve in comparatively clean water, but in a dark hole to which light had scarcely any access. Certainly the ways of rotifers are puzzling.

There was a little duck-weed in the hole, and I found, when I brought my captives home, that *Pterodina* delights in fastening itself by its sucking foot to the stem of this plant; so having by a hand lens made out a stem with a score or so of rotifers on it, I suddenly whipped it out of the water, and placed the whole party in the compressorium. By snipping off the green top of the weed, and curling the stem into a circle, I had a natural cage which held them in a moderate compass.

I do not think I ever beheld a more beautiful sight than that which the $\frac{2}{3}$rds objective, illuminated by Ross' $\frac{4}{10}$ths condenser and B stop now gave me. From thirty to forty of these animated fairy shields of glittering glass were swimming in every direction across the field, or adhering to the plant, so as to be seen from every point of view; while some had most considerately attached themselves to the glass cover, and were as quiet as rotifers ever are.

I could not help remarking that for rotifers possessing two more than usually eye-looking red spots, they steered very badly, and came very frequently into collision with each other. When too they were holding on to the stems with their sucking disks, if one was alarmed it instantly contracted its retractile foot, and flattened itself close to the stem, at the same time withdrawing its ciliated head.

But my attention was now arrested by the fact, obvious enough even under this low power, that I had two distinct species before me. One was my old friend *Pterodina patina*, but the other was a much more beautiful creature: it had a remarkably transparent lorica ornamented round the edge with bosses placed at regular intervals like those on an ancient shield, and there were two powerful transverse muscles which I had never seen in *P. patina.* A moment or two afterwards I saw, to my astonishment, one of the new species sailing by, with its lorica folded down just like the flaps of a Pembroke table, so altering its outline that it scarcely seemed to be the same animal. A friend who was sharing my delight in this novel exhibition, saw the rotifer sailing first with one flap (if I may call it so) folded down and then the other; but this I was not fortunate enough to see, though I have seen *P. valvata* (as I propose to term it) frequently with both folded at once.

Both species lived in my miniature tank for nearly a fortnight, and swarmed so that parts of its sides looked like frosted glass: but I noticed that they always avoided the light, and that when I purposely placed the tank with one end in a dark corner, that corner soon contained nearly the whole of them.

As I had hoped, there always were some among the number placed simultaneously on the compressorium, that soon adhered to the upper glass; and upon these I could bring down the higher powers.

The most striking peculiarity of the new species is the presence of the large transverse muscles for folding the lorica. The lorica is oval and nearly plane, except on its under-surface, along its major axis; where it carries a sub-conical case, in which lie the greater part of the softer portions of the rotifer.

The base of the cone is the opening from which the rotatory head is protruded, and the lorica is here slit as shown in Fig. 5, to give free play to the head, while the muscles (Fig. 1, *n*, *n*) close the flaps of the slit when the head is drawn within the lorica. At a distance from the head of about two-thirds of the length of the lorica, there is a circular opening through which the false foot is protruded and withdrawn.

The water vascular system with three tags on each side can be plainly seen; but there is no contractile vesicle. There are, however, two objects (Fig. 1, *c*, *c*) which appear to be expansions of the canals, and possibly answer the purpose of the contractile vesicle: I cannot say, however, that I ever saw them contract.

Just below the mastax are two pear-shaped glands (Fig. 1, *p*, *p*) on slender stalks attached to the œsophagus, and below these half-a-dozen or more (Fig. 1, *q*) crowding round it just above the stomach.

The gastric glands (Fig. 1, *g*, *g*) are most singular in length and shape, and are attached to the lorica at their larger ends.

The foot is divided into two distinct parts: the upper transversely wrinkled and quite flexible, the lower stiff and smooth, reminding me of a glass tube attached to an india-rubber one.

The foot ends in a curious hemispherical cup, which is lined with long cilia extending beyond its margin, and visibly in action as the animal swims. I can conceive of no function that they can perform, except that of keeping the cup free from extraneous matter which might interfere with its power of adhesion.

The Figure 1 is that of a young female, and the ovary (Fig. 1, *o*) in consequence has an insignificant appearance, whereas at a later stage it frequently obscures a large portion of the other organs.

The teeth are those of *Melicerta*, and the eyes similar to those of *Triarthra longiseta*; while the ciliated head has the two parallel rows of cilia (the upper coarse, the lower fine), with the groove between leading to the mouth, which are to be seen in so many of the larger rotifers.

PROGRESS OF MICROSCOPICAL SCIENCE.

Detection of Consumption with the Microscope.—In the 'Boston Journal of Chemistry' for December, Dr. J. G. Richardson, microscopist to the Pennsylvania Hospital, has an important note in which he fully bears out the views of our London physician, Dr. Fenwick. He says that according to his own observations he has found it useful to direct the patient to use no tobacco, to rinse out his mouth after meals before expectorating into his cup, and to avoid mixing the sputum of any other person with his own; in general, he has boiled about an ounce of the tenacious sputa with their own bulk of liquor sodæ in a four-ounce porcelain capsule or evaporating dish, and, when liquefied, poured the fluid into a conical vessel containing two or three times its bulk of water, quite slowly, to avoid cracking the glass. Although it is by no means easy to describe the appearance of lung tissue without the aid of drawings, perhaps most of his readers may be able to detect its presence by the following characters, especially if they will take the trouble to mince up a piece of healthy or tuberculous lung, and examine it as above directed after boiling in caustic soda. Under a power of 200 diameters, the fragments of the pulmonary air-vesicles appear to be composed of curved and curled fibres, each about the diameter of a horse-hair, and of a shining bluish-white colour, resembling that of the fascia lata in the thigh; their most characteristic peculiarity (observed in some part of a majority of the specimens) is the arrangement of two or more fibres in the shape of a capital Y, with a third filament crossing from the extremity of one arm of the letter to the other, thus presenting the appearance of being the meeting point of the walls of *three* air-cells, which, when enough of their outline remains, are each seen to have been from an inch to an inch and a half across. The novice must be on his guard against mistaking for lung tissue, first, small fragments of flax fibres, which, when partly split, often assume the Y shape, but without the cross-bar; second, masses of *Leptothrix buccalis* from the mouth, whose component filaments do not appear coarser than the finest hair from an infant's head; third, portions of vegetable structures, whose cells are generally smaller, while their fibres are larger and less sharply curled; and, fourth, wrinkles in the cell walls of boiled starch corpuscles, which may be detected by very close scrutiny, or by bringing the remainder of the cell into view, by means of tincture of iodine, or aniline solution. In addition to these suggestions, the following remarks, quoted from his 'Hand-book of Medical Microscopy,' p. 210, may help some observers to escape mortifying blunders of this nature:—"After much careful investigation of various specimens of sputum from both hospital patients and cases in private practice, for the purpose of detecting some characteristics of the lung tissue by which it could be promptly and certainly recognized, it occurred to me that the fibres of the air-vesicles, being elastic, must break, like a thread of india-rubber, with a square transverse fracture, while the filaments of any inelastic material, whether vegetable or animal, would *fray out*, as it were, and present to the eye a more or less obscurely pointed appearance. Further observation proved my hypothesis to be correct in numerous instances; and I believe that this characteristic of abruptly broken fibres will be found one of the most useful means yet suggested for the recognition of pulmonary tissue in sputum." In conclusion, he remarks that this plan for the early detection of phthisis is especially useful in confirming the diagnosis of obscure and otherwise doubtful cases; in distinguishing examples of bronchitis in the upper lobes of the lungs, where the physical signs simulate those of tubercular deposit; and, according to his own observations, in recognizing those masked cases of acute phthisis which at first so closely resemble enteric (typhoid) fever, that they have hitherto often misled for a time even the most astute practitioners.

Cancer of the Lymphatics.—Microscopical Appearances.—In the 'New York Medical Journal' for November, Dr. Whitall says that most of the lymphatic glands, the left breast, and the surrounding indurated tissue, contained an abundance of fibrous tissue, in which were imbedded free nuclei and nucleated cells of various shapes and sizes. In some of the glands, and in a portion of the pancreas, the cells predominated over the fibrous stroma. The central portion of the various growths was in an advanced state of fatty degeneration; in some places scarcely anything but fat was discovered; in others the cancer-cells were more or less filled with oil-globules. Portions of the pectoral muscles were reduced to mere fibres infiltrated with cancer-cells, but contained little fat. No suspicious elements were found in the stomach or in the nodule of the spleen. The liver-cells were large, many of them hyaline and without a nucleus, others nearly normal. A good deal of free oil, but not an abnormal amount, in the cells; no excess of fibrous tissue. Some of the tubes of the kidney were infarcted with granular and fatty epithelium; many of them healthy. No abnormality noticed in the tufts. There was a considerable excess of fibrous tissue.

Embryology of Limulus Polyphemus.—In a recent number (vol. iv.) of the 'American Naturalist,' there is given with many illustrations a most valuable paper on the above subject, by Dr. A. S. Packard, jun. The paper is of considerable length, but we can give an abstract to our readers. The eggs are laid in great numbers loose in the sand, the male fertilizing them after they are dropped. This is an exception to the usual mode of oviposition in Crustacea; *Squilla* and a species of *Gecarcinus* being the only exception known to the author to the law that the Crustacea bear their eggs about with them. Besides the structureless, dense, irregularly laminated chorion, there is an inner egg membrane composed of rudely hexagonal cells; this membrane increases in size with the growth of the embryo, the chorion splitting and being thrown off during the latter part of embryonic life. Unlike the Crustacea generally, the primitive band is confined to a minute area, and rests on top of the yolk, as in the spiders and scorpions, and certain Crustacea, *i. e. Eriphia spinifrons*, *Astacus fluviatilis*, *Palæmon adspersus*, and

Crangon maculosus, in which there is no metamorphosis. The embryo is a Nauplius; it sheds a Nauplius skin about the middle of embryonic life. This Nauplius skin corresponds in some respects to the "larval skin" of German embryologists. The recently hatched young of Limulus can scarcely be considered a Nauplius, like the larvæ of the Phyllopoda, Apus, and Branchipus, but is to be compared with those of the trilobites, as described and figured by Barrande, which are in *Trinucleus* and *Agnostus* born with only the cephalothorax and pygidium, the thoracic segments being added during after-life. The circular larva of *Sao hirsuta*, which has no thorax, or at least a very rudimentary thoracic region, and no pygidium, approaches nearer to the Nauplius form of the Phyllopods, though we would contend that it is not a Nauplius. The larva passes through a slightly marked metamorphosis. It differs from the adult simply in possessing a less number of abdominal feet (gills), and in having only a very rudimentary spine. Previous to hatching, it strikingly resembles *Trinucleus* and other trilobites, suggesting that the two groups should, on embryonic and structural grounds, be included in the same order, especially now that Mr. E. Billings has demonstrated that Asaphus possessed eight pairs of five-jointed legs of uniform size. The trilobate character of the body, as shown in the prominent cardiac and lateral regions of the body, and the well marked abdominal segments of the embryo, the broad sternal groove, and the position and character of the eyes and ocelli, confirm this view. The organization and the habits of Limulus throw much light on the probable anatomy and habits of the trilobites. The correspondence in the cardiac region of the two groups shows that their heart and circulation was similar. The position of the eyes shows that the trilobites probably had long and slender optic nerves, and indicates a general similarity in the nervous system. The genital organs of the trilobites were probably very similar to those of Limulus, as they could not have united sexually, and the eggs were probably laid in the sand or mud, and impregnated by the sperm-cells of the male, floating free in the water. The muscular system of the trilobites must have been highly organized, as in Limulus, as like the latter they probably lived by burrowing in the mud and sand, using the shovel-like expanse of the cephalic shield in digging in the shallow palæozoic waters after worms and stationary soft-bodied invertebrates, so that we may be warranted in supposing that the alimentary canal was constructed on the type of that of Limulus, with its large, powerful gizzard and immense liver.

American Microscopy.—At the last meeting of the American Association, that body taught the British Association a useful lesson in forming the subsection of Microscopy. In this department much valuable work was done, which we shall occupy the rest of our number in reporting:—

A New Form of Binocular Microscope, by President F. A. P. Barnard, of Columbia College, N.Y., who described elaborately a newly contrived instrument in which the light is separated into two pencils by double refraction, and which cannot fail to be a valuable addition to the resources of the working microscopist.

On the Illumination of Binocular Microscopes, by Dr. R. H. Ward, of Troy, suggested convenient means of regulating illumination in the naturalist's every-day work with the microscope, and urged that professional microscopists make their influence more distinctly felt in regard to the lower classes of instruments that are furnished to beginners, and particularly in regard to popularizing the Binocular Microscope.

Photographs, by Dr. Maddox, of the Podura Scale.—President Barnard, in exhibiting these, gave an exhaustive review of the discussion in regard to the structure of the scale. The traditional "note of exclamation," or goose-quill markings, are unlike those of any other known scale, and many naturalists are anxious, on grounds of analogy, to get rid of them. Mr. Beck argued that these marks represented parallel lines on different sides of the scale, crossing each other at an acute angle, and necessarily imperfectly focussed; some observers have attributed them to corrugations or folded ridges of the upper and lower membranes of the scale; and Dr. Pigott, with his aplanatic searcher, and others have seemed to resolve them into bead-like rows of spherules, between two membranes. The use of reflected light to determine these points is very desirable, but difficult with sufficiently high powers. Professor Smith, of Kenyon College, proposed to make the objective its own illuminator. Others have replaced the mirror he placed behind the lenses by a plate of glass or a prism; but all these means give a glare of light by reflexion from the surfaces of the lenses. The speaker had proposed a concave mirror behind the outer pair, an internal Lieberkuhn, which works exceedingly well with medium powers, say one-third or one-fourth inch; but there is not room for its insertion in high powers. As compared with Tolles' prism, which is similarly situated (above the front pair), it gives more light, and illuminates from any part or all parts of the circumference at will; on the other hand it is less easily applied, requiring the front lens to be mounted in glass instead of brass, and it is inapplicable to large opaque objects. The beaded appearance has not yet been satisfactorily seen by reflected light; nor is it well shown in the photographs where the wedge-shaped dashes seemed rather marked by cross-lines or partial interruptions. The speaker evidently doubted the accuracy of the exclamation points, but was not yet ready to accept the beads. Appearances best seen by pushing an objective far beyond its ordinary power were received with general distrust. In the discussion which followed the reading of this paper, Dr. Ward remarked that the production of a beaded appearance, as a purely optical effect, should be considered no longer doubtful, but rather an occasional accident to persons using high powers. As an extreme instance, in the case of a coarse and familiar structure, he related that while experimenting upon an elater of *Marchantia polymorpha*, that beautiful double spiral was "resolved" into three rows of "beads" or "hemispheres," perfectly distinct and unmistakable, which occupied, of course, the position of the middle and edges of the spiral. They were illuminated by parallel light, very oblique under a $\frac{1}{15}$th objective of 175° worked at a power of 3000 diameters.

Diatoms thrown up by the Sea.—Mr. E. Bicknell, of the Museum of Comparative Zoology at Cambridge, Mass., exhibited some diatoms recently thrown up by the sea at Marblehead, Mass. The deposit first found belonged to brackish water, as indicated by the nature of the diatoms and the presence of fruit of the *Characeæ*. The second deposit occurred about a mile from the first, and was purely of fresh-water origin; consisting of peat with fresh-water diatoms,—*Pinnularia*, *Stauroneis*, *Navicula rhomboides*, *N. serians*, &c. These deposits were thrown up by a severe storm on the 31st of March last, and are believed to be the first fresh-water or brackish deposits known to exist under the present ocean. They seem to be conclusive proof of the recent encroachments of the ocean upon the shore-line in that vicinity.

The Test-plate of Nobert, who has now "gone to the war," and Dr. Woodward's photographs of the same, were exhibited by Dr. Ward, chiefly in the interest of that part of the audience who were not professional microscopists, and might be unfamiliar with these wonderful works of human art. Until a year or two ago the finest lines had never been seen, even by the maker of them; now they have been seen by many persons, and have been photographed. He was now satisfied, for the first time, after hearing Mr. Bicknell's description, that the Boston microscopists had seen the *genuine* lines with powers of only five or six hundred diameters. In regard to the use of photography as a test of structure under high powers and difficult circumstances, we may learn a lesson from the broad bands of light and shade in the photograph of the coarser lines, which manifestly have no resemblance to the appearance of scratches on glass as seen under suitable powers. Dr. Ward had also been investigating the effect of seeing two planes of the object at the same time with the Wenham's binocular. The eye-pieces being practically not equidistant from the objective, the corresponding conjugate foci below do not coincide. Some microscopists have attributed much of the stereoscopic effect to this fact, which, however, does not seem to contribute perceptibly (except in the lowest powers, where the angular stereoscopic effect is necessarily very small, and where this difference of planes is most considerable) either to the stereoscopic effect or to the increased distinctness of definition above and below the plane of most perfect vision.

Microscopes Exhibited.—An abundance of instruments were furnished by members to illustrate their discussions, or for the general work of the subsection. The first-class stands were mostly of the make of Powell and Lealand, and Beck, and Crouch, of London, of Nachet of Paris, and of Zentmeyer in America. The "Jackson" model of stand, with a curved arm, seems to be growing in favour in America; and it is to be hoped that those makers who have heretofore made only one style of stand will soon offer both; so that buyers can choose their style of stand irrespective of their choice of makers. In objectives and accessories, Tolles, Wales, Zentmeyer, Grunow, Spencer, Miller, and some other American makers were represented; also Ross, Beck, Powell and Lealand, Crouch, Collins, Murray and Heath, Swift and Browning, of London; Nachet and Hartnack, of Paris; and Gundlach, of Berlin. Very low-power objectives, 3 and 4 inch, were deservedly popular. The use of immersion objectives for all high powers seemed to be *assumed by all members as a settled question.* Few members, on the other hand, fall into the present fashion of high-power objectives, preferring to use lenses of $\frac{1}{15}$th or $\frac{1}{16}$th, and downward, and gain greater amplification by other means than by reducing the nominal focus of the objective.

A Micro-Telescope.—Dr. Josiah Curtis exhibited a micro-telescope, or microscope and telescope combined, made to his order by Tolles. It is an ordinary Cutter's clinical microscope, fitted with an extra tube carrying an object-glass of 1-inch linear aperture and 6-inch focus, to which objective the compound microscope acts as an erecting eye-piece. Furnished with a proper support this makes an admirable pocket-telescope, defining well at powers of forty or fifty diameters.

Mounting very Low Objectives.—Mr. Tolles had mounted a $2\frac{1}{2}$-inch lens with the Society screw on each side of the shoulder, so that it can either be screwed on in the usual position, or passed up into the body of the instrument and fastened there, giving, by approaching the eye-piece, about the power of a 4-inch lens at the usual distance. Microscopists have been accustomed to gain a lower power than could be focussed by their rack, by screwing a low objective into the draw-tube and focussing upon the object through the empty nose-piece. The new plan of a reversible mounting is more convenient, and is applicable to instruments that have no draw-tube; unfortunately it cannot be used with the ordinary binoculars. The lens, though of second class, was very good. Mr. Tolles has also arranged a 4-inch objective, in which a short working focus is obtained by a reducing lens in the rear. This reducing lens, for convenience, is mounted in a sliding tube, and gives, when pushed in, a fair 3-inch power. As a 4-inch the combination is said to be extremely good. Mr. Bicknell applies this expedient to ordinary objectives; placing in the draw-tube, instead of the concave amplifier sometimes used, an achromatic convex lens as a reducer, with which an extremely low power can be obtained with good definition, flat field, and working focus not inconveniently long. A $4\frac{1}{2}$ or 5 inch lens (solar focus) may be used. A low objective of two combinations may be divided, using one part as an objective, and placing the other in the draw-tube.

A Clinical Compressor and Microscope.—Dr. Ward had contrived a "clinical" compressor for use with the microscope of the same name. The clinical microscope is very convenient for examining mounted specimens. He had used it for years in teaching, but not much as a "clinical." A glass slide to hold the object, with a thin cover held on by capillary attraction, is well for once, but does not satisfy a busy man. It applies to too limited a range of objects; and the cover is inconvenient to carry, awkward to handle, and easy to break. He had used Wenham's compressor until lately, but that is inconvenient under the springs of the "clinical" stage. The new compressor is simple (and therefore inexpensive) and can be used with great facility both for clinical and class use, and for much of the ordinary work of the microscopist. It is reversible, except upon a large stage, in which case it would require a few pins to serve as legs. The want of parallelism is less than in most compressors, and is not inconvenient in clinical use. The two brass plates separate entirely for arranging the object or cleaning the glass. The upper plate fits into a notch filed in a ledge at the left of the lower, the centering of the two plates being secured by a pin through the lower and a notch in the upper. The screw which attaches them at the right is permanently fastened in the upper plate by a groove and a pin. It has a coarse thread, which may be cut double to screw out more rapidly, or the thread may be reversed near the centre so that it will at the same time raise the upper and depress the lower plate. Should a steadier motion be required, a spring may be riveted upon one plate to press against the other. The apparatus is adjusted for a glass of $\frac{1}{20}$th inch below the object and $\frac{1}{125}$th above, cemented upon the inner surface of the brass plates. This is strong enough to carry in the pocket safely; it can also be used with the parabolic illuminator, or with any objective or achromatic condenser except those of large angular aperture. Should thin glass be required for any purpose, a glass or tin cell of sufficient thickness to make up the difference should be cemented on one of the plates, or both if necessary, and the thin glass fastened upon the rim thus formed. Should no cell of suitable thickness be at hand, select a glass cover of the required thickness, fasten it with marine glue on one of the plates, punch out with a file the part corresponding to the opening in the plate, and then fasten the thin glass with Canada balsam upon this extemporized rim.

A Gundlach's Microscope.—Mr. E. B. Benjamin, of New York, exhibited a microscope by Gundlach of Berlin. This was a small and cheap instrument, according to the English and American standard, but really admirable for its neatness of design and finish, and its general excellence of performance.

A New Locality for Trocheta subviridis. — In February, 1869, *T. subviridis* was found by Mr. Henry Lee, F.L.S., F.R.M.S., in the neighbourhood of Horsham, Sussex. Now he has found it in another locality. In 'Land and Water' the following account occurs:—"I have now the pleasure of announcing that I have found *Trocheta subviridis* abundant in another locality, namely, on the Beddington Sewage-Irrigation Farm of the town of Croydon, Surrey, and I am informed that this leech is also to be found on the other irrigation land belonging to the same town at Norwood. The experiences of naturalists have differed respecting the capability of *Trocheta* of living for any length of time completely immersed in water. It may therefore be interesting if I record that I found it in about six inches of water, at the bottom of the great ditch which conveys the Croydon sewage on to the estate before it flows over the land, and that during the seven weeks that have since elapsed, it has lived and thriven in a bowl of water with two minnows."

NOTES AND MEMORANDA.

A New Microscopic Lamp was exhibited by Mr. J. Browning at the last meeting. We have not had time to examine it thoroughly, but it appears to be a considerable improvement on those now in use. We therefore call attention to it. We shall probably describe it in our next issue.

Croydon Microscopical Club's Soirée.—We alluded to this in our last number, but unfortunately were unable to do more. Now, however, the Proceedings have been placed in our hands, and we are enabled to give some account of the soirée of a Society which has not been a year in existence (in fact, not much more than six months), and which already numbers over 124 members, and has the good fortune of being under the presidency of Mr. Henry Lee, F.L.S., F.R.M.S., who is almost solely responsible for the labour involved in the recent conversazione. The soirée given in connection with the club on Wednesday evening, the 23rd ult., was a fashionable and brilliant affair; it was not only attended by the principal families of Croydon and the neighbourhood, but also by several persons of distinction, among whom may be mentioned the Japanese ambassador and his secretary, who appeared to take a deep interest in the large variety of objects which Mr. Lee brought under their notice. Three tables, covered with green baize, extended along the entire length of the hall, and others all along the walls, leaving, of course, sufficient space for the visitors to promenade, and examine the numerous objects of interest which were presented for their inspection. That in the centre was reserved for members of the Linnæan Society, the Royal Microscopical Society, the Quekett Microscopical Club, and the Old Change Microscopical Society. The tables to the right and left of the centre were reserved for makers of optical instruments, of whom a large number greatly contributed to the success of the soirée by their exhibition of about 75 microscopes. The tables around the sides and end of the hall were occupied by the members of the Croydon Microscopical Club. The space at our disposal will not permit us to enter into a description of the very numerous and interesting objects that were exhibited under the hundred and forty microscopes on the tables, but we may mention the names of some of the gentlemen who kindly contributed to the enjoyment of the company. Commencing at the centre table, we observed the names of the Rev. T. Wiltshire, Captain Tyler, Drs. Mundie, Matthews, Giffard, and Murie; Messrs. Loy, P. J. Butler, W. L. Freestone, Quick, W. T. Rabbits, Charters White, W. S. Kent, C. Stewart, Suffolk, Francis, Piper, Thomas, and Brown. The manufacturers in attendance were Messrs. Ross, Murray and Heath, Baker, Moginie, R. and J. Beck, Collins, Swift, Bailey, Stanley, and Wheeler. The members of the Croydon Microscopical Club exhibited in strong force. Mr. Ridley and Mr. Cooper (coachman and gardener to Mr. Ridsdale, of Coombe-lane, Croydon) exhibited six cases of English butterflies and beetles most artistically arranged; and Mr. Skinner (carpenter on the estate of W. H. Peck, Esq., M.P.), several cases, the designs forming the Prince of Wales's feathers, with the motto "Ich dien," and two crowns traced in small beetles. In addition to those we have already mentioned, there was a large number of curious and valuable articles exhibited, amongst which may be noticed contributions by the indefatigable President, by Mr. J. F. Flower, Dr. Carpenter, Mr. N. L. Austen, and Mr. W. J. Wilson. In addition to six microscopes, Mr. Lee exhibited several curious objects, amongst which were remarkably fine specimens of the *Euplectella* (Silicious sponge) and the Portuguese Bird's Nest sponge (*Pheronema Grayi*); case of Chinese insects; a chess-board, each square representing a different snow crystal as seen through the microscope, made by the daughter of Waterhouse Hawkins, Esq.; sections of fossil and recent nautilus, and many other interesting objects. Altogether the evening was a most successful one, and adequately repaid the President for the trouble he had taken.

CORRESPONDENCE.

The Scales of Lepidocyrtus.

To the Editor of the 'Monthly Microscopical Journal.'

22, Bessborough Gardens, *Dec.* 5, 1870.

Sir,—In the report of the Proceedings of the Royal Microscopical Society, in the December number of the 'Monthly Microscopical Journal,' I read that Dr. Pigott makes me to say I have seen the scale of *Lepidocyrtus* in *squares*.

Will you kindly afford me space to say that I never saw anything of the kind, and did not say so; and that I cannot understand how so wonderful a misconception could have arisen? Dr. Pigott's remark escaped my notice at the meeting, or I would at once have corrected him.

I am, Sir, your obedient servant,

S. J. M'Intire.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *December* 14, 1870.

Dr. Millar in the chair.

The minutes of the preceding meeting were read and confirmed.

The decease of the President of the Society, the Rev. Joseph Bancroft Reade, M.A., F.R.S., was then announced to the Fellows present by Dr. Millar, in the following terms:—

It is my painful duty to inform you that our President is no more. In February last he first complained of illness, which gradually increased in severity so that it was with difficulty he presided at the last meeting of the session in May; it was then his friend Dr. Richardson examined him, and pronounced him to be suffering from incipient cancer, in a place which forbad all hope of removal by operation. From this time his downward course has been very marked; he gradually but steadily lost flesh—did not suffer so much pain as I expected, and calmly sank without a struggle on the morning of the 12th.

I last saw him alive on Tuesday, the 7th, when I spent several hours with him receiving instructions with reference to various matters. He was then very much changed, and it was clear his days were drawing to a close; he spoke to me in the most calm, clear, and collected manner, and referred to his approaching end with the utmost tranquillity. He took an interest in our Society to the last, spoke of the changes likely to be made, and requested me to present in his name a microscope which is an exact counterpart of his first one made by Dolland, and presented to him by his father when he was 15 years old. With it there is the highest power $\frac{1}{30}$th made by Dolland at that time, which he had preserved; these are interesting in the history of the microscope as landmarks of its progress.

I have also to present six slides, mounted in brass by Cuthbert, of Bat and Mouse hair, *Podura*, *Lepisma* and *Brassica* scales, together with a piece of sphagnum; these are quite curiosities in their way; they should have accompanied the Objectives he gave to the Society when I presented the Amici microscope some time ago, but they could not at the time be found, and it was only on this day week Mr. Lee found them in sorting some of his apparatus for him.

I do not know that this is the place for me to refer to it, but I desire to add that a more calm, contented, and happy frame of mind than he was in it is impossible for me to conceive, and I can only wish that my last end may be like his. He will be buried on Friday, at two o'clock.

Dr. Millar then moved, and J. W. Stephenson, Esq., seconded, that this Society learns with the deepest regret the loss it has sustained by the decease of its esteemed President, the Rev. Joseph Bancroft Reade, M.A., F.R.S., who, by his scientific attainments, useful inventions, and numerous important observations, rendered great service to microscopical science; while his high character and amiable manners endeared him to all who were honoured with his friendship or favoured with his acquaintance.

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* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: Hydra—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. M. M. J.

This resolution was passed unanimously, with strong manifestations of sympathy. It was further moved and seconded by Dr. Millar and Mr. Stephenson, and unanimously resolved, that a copy of this resolution should be transmitted to Mrs. Reade, whose bereavement the Society deplores.

It was then moved by Mr. Slack, seconded by Mr. Hogg, and carried unanimously, that this meeting do now adjourn to the usual hour on the second Wednesday in January.

In moving this resolution, Mr. Slack said he was sure that the feelings of those present rendered it impossible that any ordinary business should be transacted on that occasion. Mr. Reade won the esteem and attracted the affections of all who came in contact with him, and every Fellow of the Society was anxious to manifest respect for his memory.

BRISTOL MICROSCOPICAL SOCIETY.

Oct. 19th.—The first meeting of the above Society for the winter session was held on this day, being the 298th meeting of the Society since its commencement in 1843.

After the minutes of the last meeting had been read and confirmed, one new member was elected, and another gentleman was proposed. Dr. C. T. Hudson then read a paper on his new rotifer, *Pterodina valvata.*

Nov. 16th, 1870. Mr. R. Bernard, President, in the chair.—The minutes of the last meeting were read and confirmed.

One new member was elected. Mr. W. W. Stoddart, F.G.S., F.C.S., read a paper "On the Microscopical Examination of Air, with especial reference to the Air of Bristol."

TUNBRIDGE WELLS MICROSCOPICAL SOCIETY.*

The last monthly meeting of this Society took place at Dr. Johnson's, Tunbridge Wells, on Tuesday, December 6th. The subject for discussion was "Water and its Impurities."

MICROSCOPICAL SOCIETY OF LIVERPOOL.

(*Continued from page* 335, No. XXIV.)

The Rev. J. N. Berkeley says that the spores of Bunt infect all the seeds placed in contact with them, and it has been suggested that the plant may somehow absorb the fluid contents of the spores, and so take in the virus. The subject, it is evident, is one of great practical importance, and one to which the study of microscopists may be profitably directed.

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* Report supplied by the Rev. B. Whitelock.

After noticing *Peronospora infestans*, the cause of the potato disease, the Yeast plant, and some others, he concluded by showing what the microscope had done for Fungology, and how much remained to be done.

In 1818, Dr. Withering described 564 species of fungi, few of which were microscopic. There are now 2479 species enumerated, and there are nearly one-fourth more species of fungi than all British flowering plants together. All these, he said, had been rescued from obscurity and confusion by the microscope, as most of the specific and often generic characters depended on peculiarities discoverable only by this instrument.

With a passing allusion to the Germ Theory of disease, he invited the members to direct their attention to the wide field of discovery open to them, promising to all an immediate reward in the new forms of beauty and wonder which would be revealed to them, and to the ambitious a favourable opportunity for acquiring distinction.

Mr. Chantrell read a communication on the *Œcistes crystallinus*, the result of a correspondence he had had with Mr. Henry Davis, F.R.M.S., and the author of a paper "On Two New Species of *the Genus* Œcistes, Class Rotifera."

In the Report of the Proceedings of the February Meeting of the Liverpool Microscopical Society, which appeared in the 'Monthly Microscopical Journal' for April, it was stated by mistake that Mr. Chantrell had found in the Windsor pond, at an *earlier period* "Stephanoceros Eichornii, Melicerta ringens, Œcistes longicornis, and Cothurnia imberbis," whereas it should have said, all Mr. Richter's specimens had been found, with the *exception* of the above named. [For which the reporter is alone to blame.]

The mention of the *Œcistes longicornis* being found, led Mr. Davis to make inquiry of Mr. Chantrell about this rare species, as also about the unknown "beautiful rotifer:" the mistake was explained, and sketches of this latter rotifer sent, as also of young ones, and subsequently some live specimens. Mr. Davis's first impression was that it was a variety of *Limnias ceratophylli*, perhaps identical with fig. 1 in his paper, called *Œcistes* (*intermedias*) solely to suit Ehrenberg's classification, but he thought then and knows now that it is a Limnias scarcely distinct from *L. ceratophylli;* plate 36, fig. 2, in Pritchard, is from one of Mr. Gosse's early drawings, and is very incorrect; his later figure in 'Evenings at the Microscope' is far better. Limnias seem very subject to variation, the common form of disk is double, but specimens may be taken with the constriction, slight to another variation is in the antennæ, these being mere pimples developed to respectable horns in others. The young Œcistes, when seen laterally, are very like the illustration in Pritchard, minus the antennæ or horns. Mr. Davis was under the impression that Mr. Chantrell had left out the tubes in his sketches A and B, as the tubes would have assisted him in the identification of the species.

Mr. Chantrell explained that in this warm-water pond the tubes disappear in the mature animals, the rotifer only being very slightly ensconced in the fork of the leaves of the Anacharis, a gelatinous matter filling up nearly the space between the stem and the leaf, and that it seemed more for a receptacle for the eggs than a covering for the animal, and it is only in the very young animals that the tubes are at all perfect.

The Floscularię found in this pond are nearly all destitute of tubes and are of large size; the temperature of the water either dissolves the glutinous matter of the tubes, or the animal, finding the temperature agreeable, will not trouble himself to build a tube.

Mr. Davis expressed himself quite satisfied that the specimens sent to him by Mr. Chantrell were *Œcistes crystallinus* (Ehrenberg): the fact that the tube is irregular and undeveloped is curious, but scarcely a specific characteristic, while the animal itself seems identical with *Œcistes crystallinus* pure and simple. Does not the warmth of the water influence the secretion and deposition of the tube materials? He thought Mr. Chantrell would not be able to reconcile his opinion with the account of *Œcistes crystallinus* in Pritchard (after Ehrenberg), still less with the figure in the same book. Look at Ehrenberg's description and figures of better-known rotifers, as Melicerta, Stephanoceros, &c.; note the inaccuracies and be not surprised at a few more discrepancies. Mr. Davis states that a few years ago Mr. Gosse was so kind as to help him to identify this species, otherwise he would have never guessed what the rotifers were, which at that time he took in great abundance. In the matter of the tubes or cases, Mr. Gosse's takings and his did not agree; Mr. Gosse's had nearly opaque tubes, but his were *clear* and colourless, whilst Mr. Chantrell's seem a mixture, as if the creature tried both systems and then gave up tube building in disgust.

Mr. Chantrell pointed out a marked characteristic in the *mature* specimens of the *Œcistes crystallinus,* which is the second row of cilia carrying the food in right and left currents to the gullet or mouth; this does not show in the young specimens, which are always in tubes, but as they get larger the tube gradually disappears.

Mr. Davis stated the fact that in all this division of rotifers the mouth is outside and beneath the disc; that they have all more or less developed the *second* row of cilia: it was first noticed by Professor Huxley in Lacinularia, the first row at the edge of the disc seeming to have no directive power, merely stirring up the food in the water, whilst the *second* row (outside and under disc) appears to conduct the floating matter to the gullet.

Mr. Chantrell said he was not satisfied of this and was making further investigations, and hoped at a future meeting of the Society to give the result; he had however detected the cilia on what Mr. Davis in his paper on *Œcistes intermedia*, terms the ciliated projection or chin through which the ejected food was discharged.

The Rev. W. H. Dallinger exhibited Woodward's tank microscope.

Mr. A. Higginson, M.R.S.S., exhibited some sections of tape-worm, showing the ovaria and ova *in situ.*

Mr. Thomas Higgin, some photographs of Atlantic soundings; and

Mr. T. J. Moore, the spawn of *Siredon axolotl*, an inhabitant of fresh water from Mexico.

The usual conversazione concluded the proceedings.

Brighton and Sussex Natural History Society.

October 27th.—Microscopical meeting. Mr. T. H. Hennah, Vice-President, in the chair.

Mr. R. Glaisyer announced the receipt of twelve slides for the Society's cabinet from Mr. Wonfor.

The minutes of the previous meeting being read, Mr. Hennah called attention to the facility with which sections of bone could be made and mounted by Dr. Ormerod's process, and Mr. Wonfor remarked that the process applied equally well to such objects as the stones of fruits and shells of nuts. Its great advantages over the other methods were its simplicity and rapidity; he had made and mounted sections of bone and other hard substances in twenty minutes—a great saving of time compared with the ordinary methods, which often involved hours, without giving so good results.

Mr. Hennah then introduced the subject of the evening, "Illumination," by remarking that more depended on proper illumination than on the instrument or objectives; good objectives without proper illumination failing to show a proper performance, and inferior ones being made to do a great deal. In the illumination of objects by transmitted light, whatever the apparatus employed, the rays must be parallel. By paying attention to this, almost any structure could be made out by the mirror alone, without the expensive sub-stage appliances recommended by the opticians. The angle of illumination should never be excessive, in fact should not exceed 90°; otherwise confusion resulted. Whatever the sub-stage appliance, the light should be exactly focussed on the object. For thick substances direct rays were necessary, but for thin and lined objects oblique rays were essential. The various modes of sub-stage illumination, such as the ordinary and flat mirror, the Nachet and Amici prisms, the achromatic condenser, and the Reade's prism, were each discussed, preference being given to the Amici, when the sub-stage admitted of its application.

Mr. Wonfor then described the different methods for illuminating opaque objects or for obtaining dark-ground illumination, such as the bull's-eye condenser, the Leiberkuhn, parabolic illuminator, Wenham's paraboloid, &c.

The various methods were afterwards illustrated by Messrs. Hennah, Wonfor, and Dr. Halifax.

It was announced that the subject for November 24th would be "Bone and Allied Structures."

November 10th. Ordinary meeting. Mr. Sewell, Vice-President, in the chair.—Mr. Colborne was elected an ordinary member.

The Hon. Sec. reported the receipt of the 'Annual Report and Proceedings of the Belfast Naturalists' Field Club' from the Hon. Sec., and the Annual Reports and Journals of the Quekett Club from the commencement, from the Hon. Sec. of the club.

Mr. J. Howell read a paper "On Excavations through the Post-Pliocene of Brighton."

Mr. Mayall reported on the successful issue of his invitations of the town authorities and the society to the British Association, which had named Brighton as the place of meeting for 1872.

READING MICROSCOPICAL SOCIETY.*

December 6th, 1870.—Dr. Shattle read part of a communication as to the action of electricity in the development of nerve-cells; and having referred to the observations of John Hunter, Lister, and Richardson upon coagulation of the blood, to the remarks of Lee and Pettigrew upon the spiral arrangement of nerves and layers of the heart, and to Dr. Beale's descriptions of the nerve-cells of the frog, proceeded to give particulors of his own experiments and theory.

Captain Lang exhibited selected specimens of various Diatoms obtainable in Primary Peat; the greater number being Pinnulariæ, as *P. nobilis*, *P. major*, *P. acrosphæria*, *P. viridis*, *P. gibba*, *P. divergens*, *P. interrupta*, *P. lata*, *P. alpina.*

Mr. Tatem showed *Difflugiæ*, symmetrically mounted, and Mr. Austin exhibited the mycelium of *Peziza æruginosa* in green oak-wood.

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Abhandlungen der naturforschenden Gesellschaft zu Halle. 11 Band, 2 Heft. Halle. Schmidt.

Bulletin de l'Académie Impériale des Sciences de St-Pétersbourg. Tome XV. 36 feuilles. Leipzig. Voss.

Ueber die wachsende Kenntniss d unsichtbaren Lebens als felsbildende Bacillarien in Californien. Von Dr. C. J. Ehrenberg. Berlin. Dümmler.

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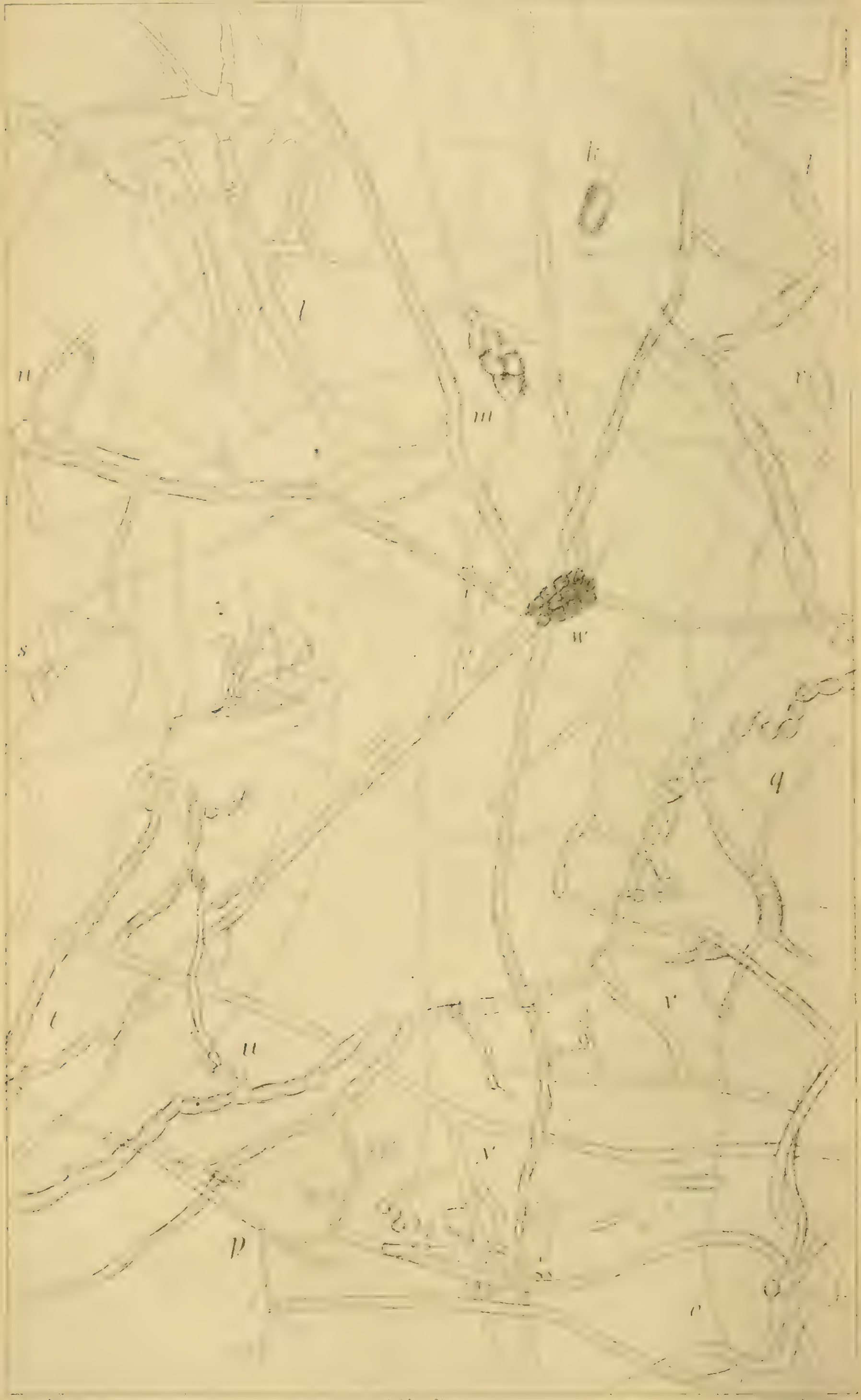
* Report supplied by Mr. B. J. Austin.



× 300 diam.

Fungi obtained from air.

THE
MONTHLY MICROSCOPICAL JOURNAL.

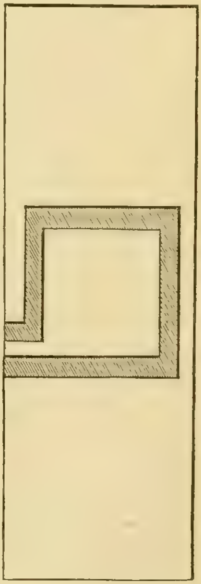
FEBRUARY 1, 1871.

I.—*Observations on the Use of the Aëroconiscope, or Air-dust Collecting Apparatus.* By R. L. Maddox, M.D.

(*Read before the* Royal Microscopical Society, *Jan.* 11, 1871.)

Plates LXXIII. and LXXIV.

The use of the instrument described and figured in the Monthly Journal of the Royal Microscopical Society, June 1st, 1870, for collecting, through the natural force of the wind, particles floating in the atmosphere, and which is now designated the Aëroconiscope, enables me to offer a brief sketch of some of the results obtained, also to extend the plan of cultivating the germs collected on the thin covers.



Wishing a more simple method of forming the air-space than with the tinfoil arrangement previously described, a preparation of mastic, wax, chloroform, &c., was made, which, by aid of a small sable-brush, would be drawn across the surface of a very slightly-warmed slide, so as to form a square, the size of the thin cover, but with one side prolonged to the edge of the slide, and the other brought nearly to meet that side, and then turned down also to the edge, so as to allow a small channel for the entrance of air. This method answered the purpose, the covers, by a very gentle pressure along the edges, remaining well fixed on the cement. Also, to form a simple damp cultivating chamber, to contain a number of slides at once, a large new ordinary writing-slate, with a well-made frame, had the latter deepened on one surface by an extra frame about one-sixth of an inch thick, and furnished with a couple of cleets at two ends, with a stop or check on a third side. After well cleaning the slate, a leaf of thick white blotting-paper was laid on the surface, and on this a sheet of cleaned pierced zinc the size of the slate tablet. The blotting-paper was damped with distilled water, the piece of pierced zinc placed on it, and a sheet of clean flat glass pushed along the grooves in the cleets at the two ends up to the stop on the opposite side of the frame, so as to form a close cover to the thin flat slate-box. When the slides had been examined and memoranda made in ink on their free surfaces, they were placed in this chamber, and exposed to the diffused light of the room. The slate-box, holding twenty-one slides, was easily managed, and the blotting-paper damped when necessary by distilled water from a dipping-tube, always trying to avoid adding too much.

The cultivating solution used for the thin covers and the slides at the commencement varied, but preference was given to the following:—

Thick clear treacle, 1 drachm;
Saturated solution of acetate of potash, 40 minims;
Distilled water, 5 drachms;

boiled in a narrow covered test-tube until reduced to 4 drachms, then closed whilst hot, allowed to cool and settle, a portion being poured off into a clean two-drachm stoppered bottle that had been previously washed out with alcohol, and dried.

Another solution was tried, which contained the same ingredients, plus 6 minims of liq. Am. fort., and boiled slowly until all trace of the smell of ammonia was absent.

A clean bent needle was employed to convey enough of the solution to smear the centre of the thin cover for about one-sixth of an inch, and to place on the slide, when set up for cultivating, a droplet of the same solution the size of a small pin head.

1st. The position chosen for the apparatus was the supposed best one the locality permitted, yet not such as to be equally exposed to wind from every direction of the compass, being somewhat protected from the S.S.W. and W. winds by the house; and as the garden is surrounded by a low wall, some diminution of the force of the wind might be expected; hence fewer particles entrapped, though with every slight breeze, from any quarter, the instrument revolved freely, the average height of the centre of the vane being five feet.

2nd. There appears to be no relation between a prevalence of germs with the wind from any one point of the compass more than another that was at all decisive, for what seemed to give such excess at one time was neutralized at others.

Nor could any relation be established between the force of the wind and the quantity of spores, though there was abundant proof of the greater increase of dust in the air when the ground was not wet. This is attributed to the light germs being carried higher than the mouth of the apparatus. A gentle breeze was the most productive.

3rd. The amount of spores collected on the surface of the thin glass varied from 250, the largest number counted, to one, and ranged through 112, 87, 60, &c., to the minor number, some days not furnishing one to be seen, on a careful examination with a $\frac{4}{10}$th objective.

4th. The prevailing spores were small pale olive-coloured oval spores, accompanied often with a small round rather darker olive-coloured spore (Figs. 8, 26, 30), both apparently some form of smut. Of the varieties of germs, &c., including chlorococcus, and what is considered not a spore, but a cellular hair of some plant, though figured, only 46 were noticed, and some of these are evidently two or more phases of the same spore. The colour in the drawings has been adhered to as strictly as possible, and the drawings made by aid of the camera lucida at 300 diameters, as also the results figured from *a* to *z*, obtained by cultivation.

The number of days of exposure was 155 (19 in April, 24 in May, 23 in June, 19 in July, 10 in August, 22 in September, 26 in October, 12 in November). The months of July and August furnished by far the largest proportion of the daily collections. The hours of exposure, except as intimated, may be taken during the long days as 10 to 11 hours, and the short days from 8 to 9 hours.

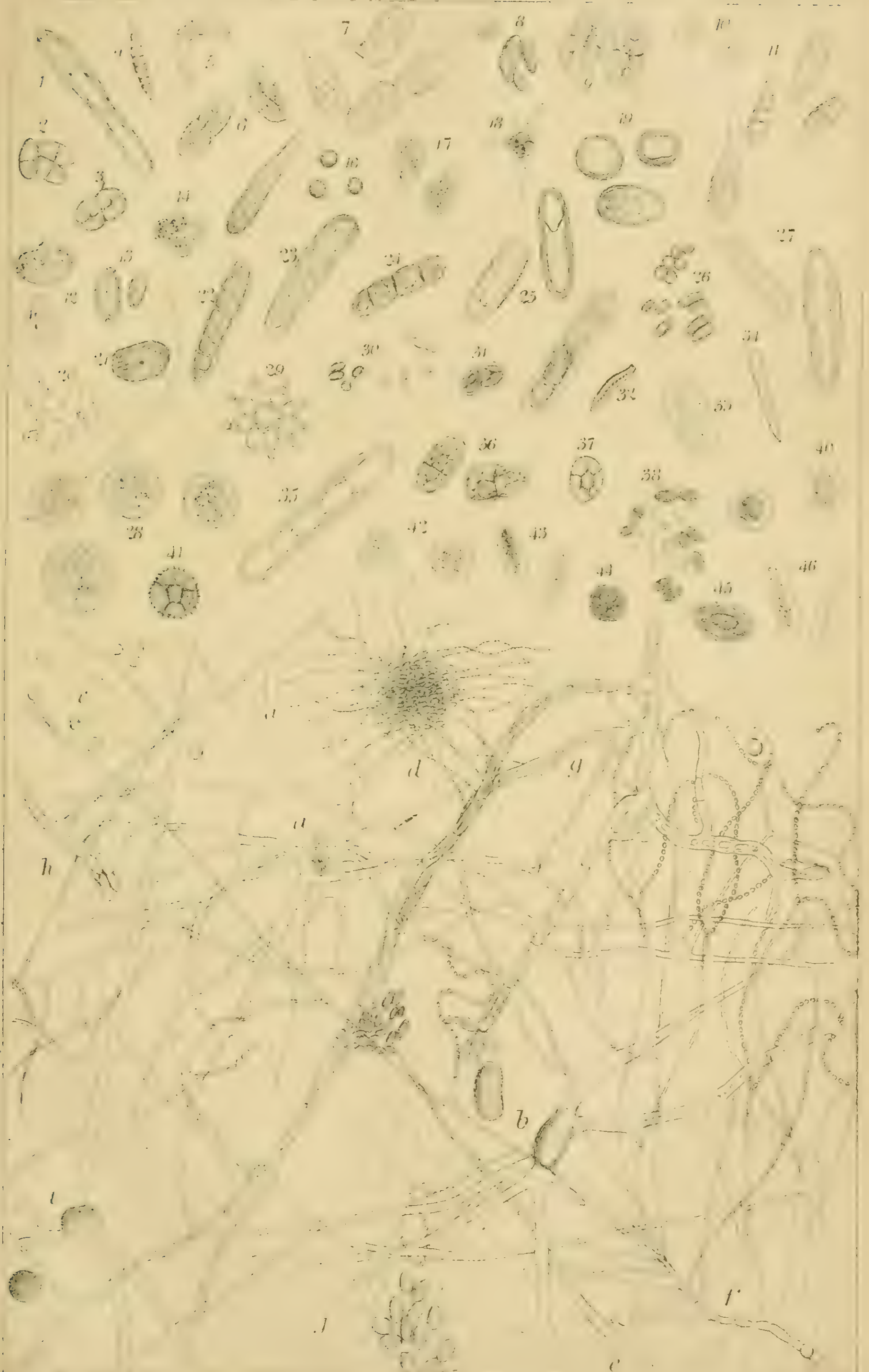
In some cases the spores were seen to commence germinating on the second day, others not before the twentieth day, and many apparently of the same appearance under the microscope and on the same thin glass did not germinate at all; nor could it be said, without trial, which was or which was not capable of growth or living. Some of the spores which germinated quickly, early put on a sickly aspect, whilst others, of apparently the same sort, grew vigorously in the same cell.

In very few cases the fungi formed their heads of spores or conidia in the open air-chamber around the droplet of the cultivating solution, as some form of penicillium, whilst others did not push their spore-bearing stems or fertile filaments beyond the edges of the liquid, though the branchlets of the mycelium were often extended to the edge of the cell. In some cases when the little drop of fluid had receded from a thread and left it exposed to the moist area only, it had the appearance of having its surface provided with minute rootlets or hairs, as in Fig. *u*, and if left for any time, it seldom recovered, by a change in position of the droplet or by increased moisture admitted from the cultivating box, either its original healthy condition or arrangement of the protoplasm.

During the very hot days there was some difficulty in keeping the quantity of the droplets of fluid constant by the supply of moisture in the cultivating chamber, and there was great inequality as to this in the different cells, though without any very apparent reason.

5th. Many of the cells were closed at some definite stage of growth of the contents by being cemented all round, yet the mycelium often continued to spread, hence others were purposely checked by the admission through capillarity at the small open space of the cell, of a little saturated solution of acetate of potash; but the entanglement of the mycelial threads was often so rapid, as in a short time to obscure the germinating spore under observation, and the threads so commingled they could not be followed back to their primary source. During absence from home, some days in August, also occasionally on account of the extreme violence of the wind, rain, or other reasons, the apparatus was not set up, and occasionally on account of wet was removed for part of the day: in one case it was left out all night, otherwise the exposures were only during the day-time. Once the same cover was used twice, *i. e.* on the following day. There would be little use to try and tabulate the amount of spores obtained per diem or per hour, for the apparatus embraced no method for recording the velocity of the wind, hence it would be impossible to estimate the quantity of air that traversed the instrument within a given time. Suffice that the conditions were simply those which, without any augmentation or decrease, any person would be exposed to on the same spot and turning to the course of the wind, though with the difference between the areas of the breathing apertures and the open or arrow end of the vane. The results show that the chances if an aspirator had been used on various and not successive days, or even upon parts of the same days, that no reliability as to the natural quantity of dust or spores at that height from the ground would have been obtained, and the present report only furnishes the amount of those gathered in the day-time, except on one occasion, hence it is necessarily incomplete for the twenty-four hours. The septate spores (Fig. 11, and *m*) were of frequent occurrence and germinated freely, but without furnishing any secondary spores. Whilst in various cultivating cells some minute bodies, figured at *d*, *o*, and *s*, suddenly as it were made their appearance, and are I believe gonidial forms of one of the fungi or zoospores put forth under unnatural conditions: some germinated to the extent of throwing out fine branches of mycelium, which I think is not usual, as in Fig. *d*. The minute bodies, figured at *o*, had a *slight* movement, but no cilia could be detected, nor had they the form of bacteria or monads, but were most likely gonidial bodies emitted by the terminal, though free conidia, seen near to them.

To conclude this imperfect sketch of the result of the employment of this form of aëroconiscope, it may be briefly said that it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fulfilled its purpose well, was exceedingly easily managed, and might, if connected with the usual meteorological instruments at our observatories or other institutions, gradually enlarge our knowledge of the chances of such or such germs being either directly or indirectly related to those atmospheric conditions usually associated with various epidemics. The employment of this one has afforded results sufficient to establish the utility of the apparatus, which, if for continued use, should by preference be made of thin sheet copper or iron, enamelled on both surfaces, especially the inner one, to diminish the chance under light breezes of the dust, &c., resting against the inner surface in the transit of the air through the instrument. Also that it should be exposed day and night, protected, if possible, from the rain. If found too heavy to rotate easily, though, if nicely balanced, there would be little chance of this, the side funnels might be omitted; yet as the apparatus revolves sometimes rapidly, or in advance of the direction of the wind, they are useful. If absent, it would be almost requisite to limit the rotation by a spring pressure that should be called into play according to the force of the wind, so as to steady the vane more or less, yet leave it free to rotate under less wind, or more gentle breezes, without any hindrance.

Note.—No attempt has been made to name the various forms gathered from the air or the results obtained by their cultivation, as the abnormal conditions under which they were produced might have considerably altered their common or natural appearance. It has been deemed best to simply give the results in figures for comparison, in case this interesting study be earnestly pursued by others; yet the absence of colour in the Plates will render even this more or less difficult,—for many of the spores are so nearly of the same size and shape, though differing in colour, they may easily be confounded; also the colour of the mycelium, which may help to distinguish some of the forms from each other, I found to vary considerably with the medicine in which they were grown, so that in the absence of the spore-bearing stems or heads, considerable difficulty must occur in correctly naming or comparing the figures. This will furnish some apology for the few allusions to the figures in the letterpress. The chief object being to prove the utility of the apparatus in the first instance.

II.—*On the Employment of Colloid Silica in the Preparation of Crystals for the Polariscope.*

By Henry S. Slack, F.G.S., Sec. R.M.S.

(*Read before the* Royal Microscopical Society, *Jan.* 11, 1871.)

I lately called the attention of the Society to the nature of the cracked films obtained by slowly, or quickly, evaporating a dialyzed solution of colloid silica in distilled water. As these films solidify they crack in various directions, and make complicated patterns of straight and curved lines, sometimes appearing like the plan of a city, and at others assuming curious columnar forms with transverse striations. It is also common to find slight changes in the level of different parts of the films, sufficient, as explained in a previous paper, to exhibit Newton's rings in a very beautiful manner.

It seemed probable that by dissolving crystallizable bodies suitable for exhibition with the polariscope, in a solution of silica instead of in plain water, some interesting modifications of crystalline patterns might be obtained. Using sulphate of copper in this way afforded the following results. First, what may be termed a "pavement pattern," in which the silica cracks divide the film into a great number of small compartments of three, four, five, or more sides, which may be roughly likened to the appearance of vegetable ivory, or a section of pinna shell. In each of these divisions crystallization takes place, usually radiating from the centre, and sufficiently diversified in thickness or position to produce polariscope patterns that remind me of a richly and variegated pavement, deviating from "tesselation," inasmuch as the compartments are not squares. The crystals thus reduced have a tendency to exhibit Maltese crosses of colour, changing with the positions of the polarizing and analyzing apparatus.

In these "pavement patterns" the force exerted by the silica films in contracting and cracking, causes the crystalline force to operate in limited compartments without change in the linear direction, common to radiating groups.

In other cases the contractile force acts tangentially, or in curves, while the crystalline force operates radially, and then we obtain exquisite spiral and turbinate forms, very similar to those which Mr. Thomas brought before the notice of the Society in 1866, and which probably resulted from an analogous combination of contractile and radial forces operating so as to give curved resultants. If a number of trials are made with different modifications of the temperature and times of drying, all sorts of conflicts between the two forces will be found to occur. One very instructive instance supplied a pattern commencing with circles filled with radii, and marked with concentric rings, after which the radii became spirally twisted and plumose, and terminated in straight lines, regaining their normal direction. Several of the gyrating portions had the appearance of spirally twisting about their own axes. This aspect probably resulted merely from the arrangement of colour, light, and shade obtained by the action of polarized light.

In other cases feather and landscape effects were obtained, owing a good deal of their richness and diversification to the silica cracks and changes of level.

By dissolving salicine in the solution of colloid silica—aiding the operation by heat—something like the pavement pattern of the copper slides was occasionally obtained, each compartment being filled with a beautiful group of crystals assuming the shape of a pink, or some such flower. At other times the circular and radial patterns so well known to microscopists were modified by the radii becoming plumose, and more or less curved. In a few instances a very elegant fan-shaped and extremely delicate crystallization sprang from solid crystals like fan-handles, the fan portion being decorated with concentric rings, and terminating in a border of fine needles, looking like a silken fringe, and appearing under suitable illumination to be in a different plane from the fans themselves.

Hippuric acid similarly treated yielded flower patterns of great beauty, some of them taking the form of what the botanists call a ringent corolla. These are usually of extreme delicacy, and only seen well in one or two positions of the polarizing apparatus.*

Tartaric acid used in the same way gives irregular pavement patterns, some of the compartments looking flat, and of more or less uniform tint, while others appear slightly raised and ribbed, bearing some resemblance to coloured acorn-barnacles. The actual variation of level in these cases is very slight; far less than what the perspective action of shade and colour would make it appear.

All these crystals require careful management of the polarizing apparatus, and a selenite stage to bring out their full beauty. The "neutral tint stage" of Mr. Ackland shows them extremely well. When properly displayed it will be found that crystals prepared in this way afford a greater range of colour than is common, and the artistic defect arising from too violent contrasts is avoided. The observer should not be satisfied unless the primary tints are associated with a sufficient variety and quantity of secondary and tertiary tints to produce a true chromatic harmony.

It is impossible to view a number of slides prepared in this way without being struck with the resemblance of many of the patterns to those of sections of various structures in organized beings, and we are led in the direction long since indicated by Mr. Rainey to extend our notions of the functions performed by the chemical and physical forces at work, according to their own methods, in living things. The action of the silica in the cases cited seems to be chiefly physical and mechanical, but the appearance of the crystals, especially when viewed under considerable magnification, leads to the belief that some portion of the silica is present in them and not eliminated as the process of crystallization goes on. Other methods have been long practised by microscopists to modify crystallization, and which appear to depend upon the same principle of offering resistance to the normal action of crystalline force. One advantage of employing silica, as described, is that we can obtain a number of interesting patterns with films of that substance alone, and compare them with the varieties of crystalline patterns obtained from simple aqueous solution, and with those producible from the mixed solutions. Fellows of the Society experimenting in these directions, will do well to examine the beautiful slides of various crystals prepared by Mr. Norman, which will be found in the Society's cabinet.

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* Quite different patterns were obtained on other occasions. The author hopes shortly to bring before the Society, and publish in this Journal, illustrations of some of the most remarkable forms obtained with different bodies.

III.—*The Development of Phycocyan.*

By Thomas Charters White, M.R.C.S., L.S.D.

(*Read before the* Royal Microscopical Society, *Jan.* 11, 1871.)

Plate LXXV., Upper Part.

There are many present, doubtless, this evening who may remember the interesting specimen of Dichroic Fluid exhibited upon two occasions by our late esteemed President—one obtained at Canterbury, by Mr. Sheppard, and the other by Mr. B. D. Jackson, from a pond at St. Leonards; these fluids, as you may remember, presented the peculiarity of looking red by reflected light, and blue by transmitted light. I do not know if either of these gentlemen saw the process of development of this Dichroism, but I have had the opportunity of watching its gradual growth, and desire in this short communication to lay before you its life history as I observed it, and then leave to others the explanation of its origin and the theories deducible from it.

About the early part of November, having some friends coming to spend a microscopical evening, and being desirous of exhibiting something living for their amusement, I sent to a favourite hunting ground of mine, the Round Pond in Kensington Gardens, where often floating portions of Myriaphyllum and such-like aquatic plants afford abundant game for the microscopist; but, alas! Boards of Health, and such-like bodies, being inimical to everything but cleanliness, had shot in plenty of clean gravel, and no weeds could be detected: where Floscularia and Bacillaria had before been found in plenty, nought could be gathered but some green flocculent matter, which settled at the bottom of the collecting bottle like green cumuli clouds. Upon placing some of this under the microscope, the green matter was seen to be made up of moniliform strings of cells, coiled up to the extent of a turn and a half, and of a pale green colour: mixed with these were several bundles of fibres of an ochreous colour, looking like dead bundles of *Bacillaria paradoxa*, arranged in a slightly fan-shaped form (Fig. 2): animal life appeared almost absent, and was represented by a few monads. Not thinking this worth exhibiting to my friends, I screwed down the top of my York bottle, and stood it on the window-ledge inside my room, and it was there forgotten for about a fortnight, when by chance looking up at it, I saw that the green flocculent matter had descended to the dead level of some yellowish-coloured mud, but for about a quarter of an inch above it was a layer of the deepest richest indigo blue I had ever seen. Taking it down carefully, so that no disturbance of it should take place, I looked at it by reflected light, when it was a rich crimson lake. I then remembered the Dichroic Fluids

I had seen here, and in which the late Reverend President had taken such an interest, and I made a careful microscopical examination of this specimen, and communicated the following results to him. On removing the stopper of the bottle an opaque semi-gelatinous film covered the surface of the water: upon examination it appeared full of minute granules; while round its semi-globular masses, Paramecia in every size and stage of growth were sporting and voraciously feeding (Fig. 1). The layer of blue contained no life, nor any particles of an appreciable size, while the mud seemed nothing else than the disintegrated particles of the green spirals of a fortnight ago. After the uncorking of the bottle and this microscopical examination the blue colour became diffused, and the Dichroic character of the fluid became like that brought to this Society. I wrote to the late Rev. Mr. Reade, giving him this life history of Phycocyan, and at his wish I bring it before the notice of the Fellows. About a fortnight after this I examined the fluid, when it teemed with Vibriones; all the Dichroic appearance had gone, and the Paramecia had disappeared, but it had been closely corked, and having no vegetation to aërate the water, they were probably killed. Upon a recent examination of the sediment abundant Amœbiform bodies were seen in all sizes, and animal life of a low form was very abundant, but for the last week or so the bottle has remained indoors uncorked, thus affording air for the development of life. I leave the explanation of this colour to others, and only state the facts as they appeared to me.

BISHOPSBOURNE RECTORY, *Nov.* 19, 1870.

MY DEAR MR. WHITE,—Pray send a note without delay to the Microscopical Journal, describing your interesting experiment upon Phycocyan, which entirely confirms my views and Mr. Sheppard's as described at the last meeting. The fluid consists, as you also show, of opaque red particles and the blue solution. Your coloured solution was formed without the addition of albumen, as was also Mr. Sheppard's in his first experiment, where the albuminous eggs supplied what was wanting, as is the case in your experiment: so we may say that the *simplicity* of Phycocyan depends upon its *duplicity*.

I return your two drawings, that you may have them engraved on wood. With many thanks,

Believe me very truly yours,
J. B. READE.

(P.S.—You might send this note to the Journal, as a preface to yours. I am so weak that a friend writes for me.—J. B. R.)

The Monthly Microscopical Journal. Feb 1. 1871. [illegible]

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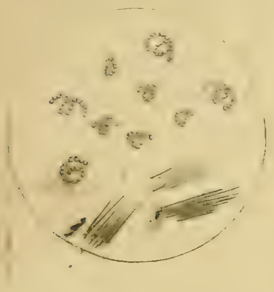

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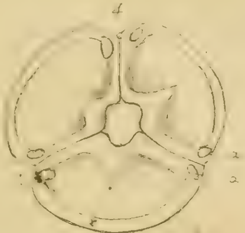

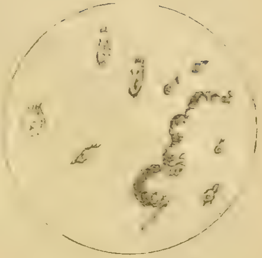

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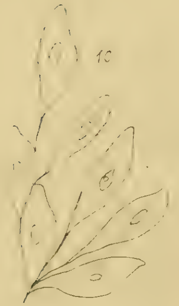

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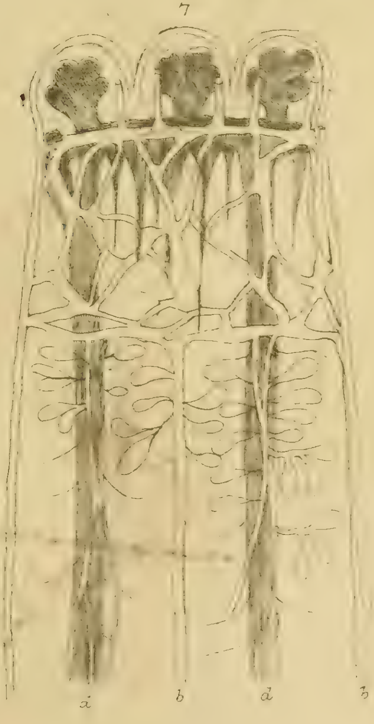

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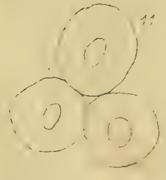

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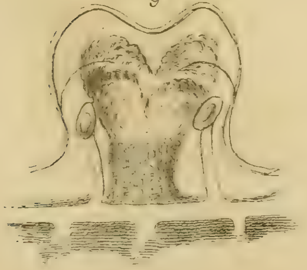

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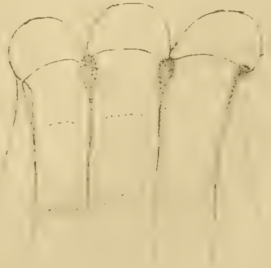

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IV.—*The Anatomy of the Round-worm.*
(*Ascaris lumbricoides*, Linn.)

By B. T. Lowne, M.R.C.S. Eng.

(*Read before the* Royal Microscopical Society, *Jan.* 11, 1871.)

Plates LXXV. and LXXVI.

The anatomy of the Nematoida still remains in the greatest obscurity, although Cloquet, Otto, Meisner, Schneider, Dr. Bastian, and others, have contributed largely to the literature of the subject. Numerous contradictions are found in their works, and much that is unsatisfactory remains to be cleared up.

About four months ago I was fortunate enough to obtain fifty or sixty large round-worms from a patient, who was the host of nearly two hundred of these parasites. I thought this was a favourable opportunity to endeavour to clear up the more important points in the anatomy of the Nematoid worms. The result of my investigation has been most satisfactory to myself; and although I differ in my conclusions from all previous writers, I have not hesitated to publish the results of my inquiry, as, if I am right, my observations throw an entirely new light upon the anatomy of the Nematoids, and establish their place in the animal kingdom in a most satisfactory manner.

The *Ascaris lumbricoides* resembles an earth-worm at first sight. The female varies from ten to sixteen inches, and the male from five to eight inches in length. The extremities are tapered; the anterior extremity being more so. The alimentary canal runs straight through the animal, the mouth and anus being situated at the extremities. There is a distinct space between the body walls and the alimentary canal,—the body cavity,—filled with a pink serum-like fluid, in which the organs of generation lie. This fluid keeps the external walls tense, and escapes with a gush when they are ruptured. The duct of the female generative organs opens on the ventral surface, at the junction of the anterior and middle thirds of the body: that of the male has its orifice very near the posterior extremity. A pair of curved spicules, retracted when at rest, constitute the armature of the male generative aperture, one being placed on either side of the opening.

The body wall consists of the integument and of a thick layer of longitudinal muscles beneath it.

The integument exhibits two diverse layers, an external chitinous and transparent, and an internal cellular portion. The external layer is indistinctly segmented, exhibiting many hundred annuli. This layer is apparently structureless, but may be split up into numerous layers, a fact first pointed out by Czermack. It presents no trace of cellular origin, and I am inclined to look upon it in the same light as Dr. Cobbold, as being probably an excretion of the inner cellular layer, which consists of small granular-looking cells.

Immediately beneath the integument is a thick layer of longitudinal muscles. The muscular fibres (Plate LXXV., Fig. 3) are about $\frac{1}{500}$th of an inch in diameter, non-striated, nebulous, and occasionally exhibit nuclei at intervals. They are dissolved rapidly by moderately strong acetic acid, and, like all the tissues of the Ascaris, are very elastic.

I have been unable to detect any transverse layer of muscle, although Cloquet has described such a layer, and I suspect with Dr. Cobbold that the annular striation of the integument has given rise to the opinion that such a layer exists.

The mouth is surrounded by three lips, emarginate in front (Fig. 4). The external chitinous layer of the integument is continued into the thick muscular pharynx, which it lines. It is thrown into three ridges which alternate with the lips (Fig. 5), and which are covered with sharp-pointed tubercles, forming three very effective pharyngeal teeth. Fig. 6 represents the rounded anterior extremity of one of these ridges. The exterior of these ridges, which are prolonged to the posterior extremity of the pharynx, gives attachment to the transverse muscular fibres of the pharynx, so that these are divided into three sets.

Several slighter ridges run transversely across the main ridges, and probably give attachment to some of the numerous longitudinal muscular fibres of the pharynx. The fibres of the pharynx are less than half the diameter of those of the body wall, but are, like them, non-striated.

I have been unable to detect any muscle in the alimentary canal below the pharynx, which appears to consist of an external chitinous layer lined with epithelium. In this respect it is precisely like the inner layer of the intestinal canal of an insect. I strongly suspect that the rough surface of the ridges of the pharynx is epithelial, and that the pharynx is elsewhere lined with epithelium similar to that in the remainder of the intestinal canal, but I have been unable to observe this satisfactorily.

I would point out some remarkable affinities with the abranchiate annelida, exemplified in the three pharyngeal teeth and the segmentation of the integument, which is very similar to that in the leech: these seem to indicate a closer affinity than has hitherto been recognized.

One of the first points which attracts the attention of the most casual observer in the external appearance of Ascaris is the presence of four longitudinal white or pinkish lines or bands running the whole length of the animal. These have been incorrectly described by numerous observers as muscles, and Dr. Cobbold has fallen into the singular error of speaking of them as the "only muscles in

Ascaris," although Cloquet has correctly described the longitudinal layer of muscles. Similar bands occur in *Mermis*, *Gordius*,—and indeed, in all probability, in all Nematoids, as well as in Echinorhynchus, &c.

On careful examination it will be at once seen that two of these lines are much broader and more conspicuous than the others: these are lateral in position. The narrower lines are dorsal and ventral, the ventral line in the female deviating from its course and passing on one side of the sexual orifice, not dividing and surrounding it, as is stated by Cloquet.

The alimentary canal behind the pharynx in the anterior portion of the body is stretched across from one of these lateral bands to the other, but in its posterior two-thirds it is merely suspended in the body cavity and surrounded by the convolutions of the ovaries in the female, and of the testis in the male.

This condition of the anterior portion of the alimentary canal is of considerable importance when viewed in relation to the condition of the bands and alimentary canal in Mermis. In Ascaris I have failed to trace any nearer connection between the alimentary canal and the lateral bands than, that the alimentary canal is connected with the body walls at these points by connective tissue of extreme tenuity. I am inclined to the belief that the whole body cavity is lined with an extremely thin membrane which is reflected upon the intestine at these points. It will be necessary hereafter to refer again to this relation of the lateral bands and alimentary canal.

The anus is situated at the posterior extremity of the body, and the alimentary canal is usually filled with dark-coloured chyle.

In these preparations* the whole interior of the body cavity will be seen to be covered with white villi-like processes; these are the appendices nouricières of Cloquet, and when examined with the microscope are seen to be pyriform vesicles, which are filled with a white molecular fluid. They are supported by ducts which form a complicated network over the whole inner surface of the body wall. If a transverse section of the worm be made, this network of tubes may be seen to be connected more closely with the body wall at four points than elsewhere; that is, they are connected with the four longitudinal bands above described.

The dorsal and ventral bands appear to be the main trunks of this system, and to give off lateral branches at short intervals, which support the vesicles themselves. Plate LXXVI., Fig. 2, represents a portion of one of these trunks with its lateral ducts and vesicles. A section of the worm is accurately represented in Schneider's 'Monograph on Nematoids.'

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* The preparations here referred to have been presented by the writer to the Museum of the Royal College of Surgeons.

The connection between this system of vessels and the lateral bands is somewhat different. Several longitudinal vessels anastomosing at intervals surround the lateral bands, and represent the dorsal and ventral trunks in this region. These are connected directly with the transverse vessels.

If the anterior extremity of the worm be removed about three-quarters of an inch from the head and slit up with a fine pair of scissors, by passing one of the blades through the pharynx; a preparation may be made like that represented in Fig. 7, by carefully removing the pharynx. The four sets of longitudinal vessels—which, together with the transverse vessels and vesicles, I shall no longer hesitate to call a water vascular system—will be seen to form a plexus by anastomosing with each other, which entirely surrounds the pharynx. This portion of the water vascular system is destitute, or almost destitute, of vesicles. The plexus terminates anteriorly in a well-marked ring. The ring gives off six branches which open externally one on either side of each lip. This ring, and the vessels given off from it in front, are figured by Dr. Bastian,* and described by him as nerves and a nervous ring; in this he has probably been misled by Schneider's† description, although Schneider has clearly described a totally different structure.

The external openings on the lips (Fig. 4 *aa*) are oval and have a slightly raised margin. The external integument is connected with the walls of the vessels at these points. Just within the margin of each opening there appear to be several perforations, generally two or three; but as far as I can make out they all open into a single duct. These have been described as papillæ, and have always been supposed to be sensory organs. J. H. Flogel‡ has described and figured precisely similar structures on the lips of three different species of Oxyuris, but speaks of them as papillæ over the terminations of nerves. Dr. Bastian§ admits that he has been unable to trace any nerves into the papillæ of Ascaris.

With regard to the transverse ventral slit opening into the water vascular system, which has been described again and again in Nematoid worms, I can find no such opening in Ascaris; there is but one transverse ventral slit, and that is the orifice of the female sexual organs. I have found no such opening in the male.

The transverse opening described in some Nematoids seems, as far as I can judge, to belong to some other system of organs.

If a portion of either the dorsal or ventral main trunk be carefully removed, with its transverse branches and vesicles, it will be found that there are numerous secondary and tertiary vesicles.

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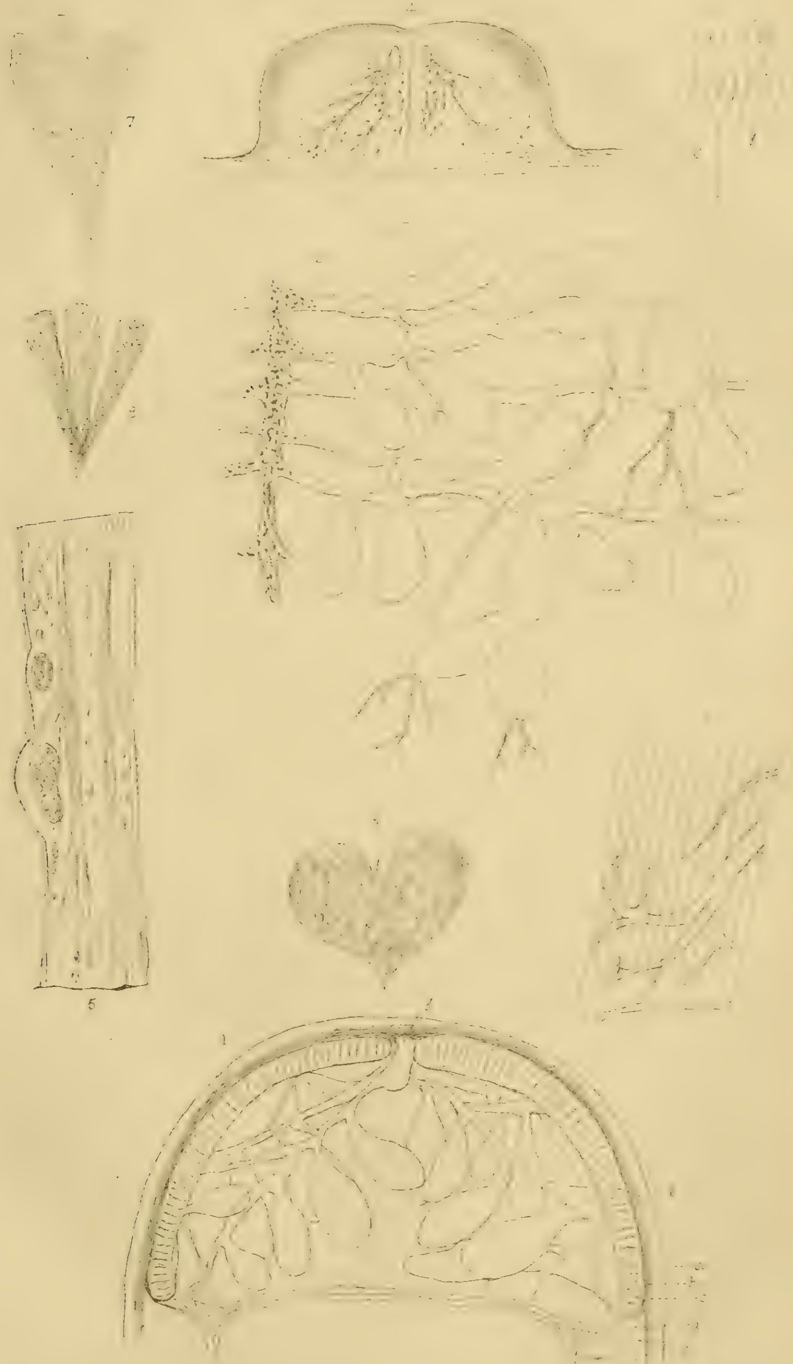
* Dr. Bastian "On Nematoid Worms," 'Phil. Trans.,' vol. clvi., p. 54.

† Schneider, 'Monographie der Nematoiden,' and 'Mull. Archiv.' 1863. P. 1.

‡ 'Köll. Zeitschrift für Zoologie,' band xix., p. 234.

§ *Loc. cit.*

The Monthly Microscopical Journal. Feb 1. 1871. Pl. LXXVI.



Tuffen West sc.
W. West & Co. imp.

Structure of the Round Worm.

These differ from the primary vesicles in opening into each other, and into the primary vesicles, just as the latter open into the transverse vesicles.

These secondary and tertiary vesicles give off minute dichotomously dividing vessels, which dip into the muscular layer, and ramify in and over all the organs of the body, forming a true pseudo-hæmal system. The main trunks, lateral branches, and vesicles of the water vascular system have an external coat of fine muscular fibres, an inner membranous coat, and an epithelial lining. The pseudo-hæmal vessels seem to consist of membrane only.

The whole water vascular system is filled with a viscid molecular fluid, and is most conveniently seen by staining the tissues with an alkaline solution of carmine, or by treating a portion of the worm with dilute acetic acid. Specimens so treated and mounted in glycerine jelly are exhibited under the microscopes.

I will recapitulate the main points, in order to avoid any chance of being misinterpreted. The water vascular system consists, according to my observations, of a ring round the pharynx, connected with pores in the outer sides of the lips, and giving off a plexus of vessels posteriorly. From this plexus two large trunks, one dorsal and the other ventral, pass to the posterior extremity of the body, and two lateral sets of smaller longitudinal vessels accompany the lateral bands.

Both lateral, dorsal, and ventral vessels give off transverse vessels, which form a plexus, and bear numerous vesicles. The vesicles open into other, and these into other vesicles, the latter giving off fine vessels, which I have called pseudo-hæmal. The pseudo-hæmal vessels ramify in and upon all the other tissues.

The similarity of the whole water vascular system of Ascaris to the ambulacral system of the Echinodermata can hardly escape notice. If the Ascaris possessed ambulacral processes in connection with the vesicles above described, the two sets of organs would be nearly identical.

The water vascular system of Ascaris seems also to afford a strong indication that Professor Huxley's hypothesis concerning the nature of the so-called pseudo-hæmal system of Echinoderms and Annelida is correct, that they really belong to the same system of tubes with the ambulacral and water vascular systems.

I further suspect very strongly that in Ascaris we see a transitional form, in which the water vascular system is immediately concerned with the function of nutrition, with a somewhat rudimentary condition of the alimentary canal, seen in the absence of a muscular coat beyond the pharynx, which is connected closely with the condition of things met with in the anenterhelminths, or worms without any intestinal canal, where, as in Tenia, the function of nutrition undoubtedly devolves upon a system of tubes homologous with a water vascular system. The remarkably anomalous condition of the alimentary canal of Mermis seems to stand between the condition seen in Ascaris, and that in Tenia, Echinorhynchus, &c.

The lateral bands present histological elements precisely similar to those described by Claparède [*] in the ventral cord (*Bauchstrang*) of the earth-worm, about the nervous nature of which there can, I think, be no doubt whatever. These are accurately figured by Schneider. [†]

Each lateral band is divided into two, longitudinally, by a well-marked raphé of fibrous tissue, and a central canal may be seen on either side of the raphé. This may be clearly observed in a transverse section. The greater part of the band consists of fine fibrous tissue, minute oil-globules, and small corpuscles, called by Claparède, in the earth-worm, connective tissue nuclei. Besides these, there are numerous larger scattered ganglion corpuscles, just as there are in the ventral cord of the earth-worm. I have distinctly traced the connection between these and the nerve filaments. The whole band is invested by a delicate layer of epithelium, exactly as that of the earth-worm is.

In the earth-worm the ventral cord terminates in a ganglionic ring round the pharynx; [‡] in Ascaris the lateral bands terminate in a similar ganglionic ring.

The pharyngeal ring gives off a thick band of similar structure to each of the lips, which terminates in four large lobes. Posteriorly the ring gives off several short tapering bands, identical in structure with the lateral bands. These are soon lost in the tissues. Occasionally one or both lateral bands split before joining the ring. Nerves are given off at intervals from either side of the lateral bands, which may be traced running transversely to the muscular layer, and ramifying in the integument.

Schneider [§] has clearly seen and described this nervous ring in *Ascaris megacephala*, and has figured the lateral nerves given off from the lateral bands; [‖] but he has failed to trace the connection between the lateral bands and the ring, and describes the nerves as coming from a nerve-trunk in the interior of the lateral bands. I suspect this has arisen from his method of treating his preparations with acetic acid, and from the fact, that the longitudinal nerve-fibres, which form chiefly the nerves, are situated in the centre of each half of the lateral bands; whilst the commissural fibres and ganglion cells are scattered over their periphery.

With regard to the nature of the axial canals in the lateral bands, I confess I am ignorant. I have a strong suspicion that they are pseudo-hæmal sinuses, from the large number of minute pseudo-hæmal vessels which appear to ramify from them. These are very apparent when a transverse section of the band is stained with carmine fluid. The disposition of the parts of the bands are represented in Figs. 4 and 5, Plate LXXVI.

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[*] E. Claparède, 'Zeitschrift für Zoologie,' band xix., p. 563.

[†] 'Monographie der Nematoiden.'

[‡] Claparède, *loc. cit.*

[§] 'Monographie der Nematoiden,' and in 'Mull. Archiv.' 1856. P. 1.

[‖] 'Monographie der Nematoiden.'

I have repeatedly observed certain oval cellular bodies, of greater transparency than the bands themselves, at intervals bulging out, and apparently compressing the normal structure of the bands. They are very irregular in their occurrence, and I have been unable to make out their import. They are very like the cellular bodies which Meisner described as so abundant in the lateral bands of Mermis. Meisner thought the lateral bands of Mermis were connected with the alimentary canal, and called them corpora adiposa. The connection between the alimentary canal of Ascaris and the lateral bands is by connective tissue only, as has been already stated; and the same is even more marked in Gordius.* The specimens in which I observed the cellular bodies had been preserved in spirit too long, but I have no doubt they are collections of ganglion corpuscles, so that they may be looked upon as true ganglia. The lateral bands contain a large quantity of oily matter, and unless examined in a tolerably recent condition it is impossible to make out their true nature.

Cuvier seems to have rightly interpreted the nature of the lateral bands. Meisner has described certain nervous cords in Mermis, but I suspect he fell into the same error as Cloquet and Otto, who described the dorsal and ventral bands in Ascaris as nerve-trunks. Perhaps Meisner has described a portion of the water vascular system as a nerve-trunk; I do not know, as I have not examined Mermis; but I cannot help believing that the bands, or some of them, for there are three, are homologous to those in Ascaris. In Gordius there is a single ventral cord, which undoubtedly represents the lateral bands in Ascaris; in this I think I agree with Schneider. It is on the ventral aspect of this cord in Gordius that Meisner described his ventral nerve-cord, but Grenacher† has failed to detect it. The last-named author thinks that the ventral cord in Gordius as well as the lateral bands in Ascaris are mere folds of the cellular layer of the integument. In this I cannot agree with him; and his beautiful drawings of Gordius, in the paper already referred to, are far from giving me the idea, that the ventral cord is so simple a structure as he is inclined to consider it. The epithelial layer covering the lateral bands may, however, be a portion of the inner layer of the integument, and this leads me to say a few words on a point of very considerable interest.

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* Dr. H. Grenacher, 'Zur Anatomie der Gattung Gordius.' 'Köll. Zeitschrift für Zoologie,' band xviii., p. 322.

† *Loc. cit.*

I am beginning to suspect that the nervous system of the invertebrata is developed, like that of the vertebrata, externally to the serous layer of the blastodermic membrane. I expect soon to be able to throw some light upon this subject; let it suffice at present to say that it is possible that the cellular layer of the integument of Nematoids may represent the serous layer of the blastodermic membrane, and this is apparently folded so as to invest the lateral bands. The ento-thoracic growths of the integument in insects point to the same conclusion. But this is mere theory with our present knowledge.

From my observations I conclude that the nervous system in the Scolecida is not formed on so variable a type as imperfect observations have hitherto led us to believe; but that it is entirely transitional between that of the Annelida and Echinodermata.

In the Echinodermata we find a ganglionic ring around the pharynx giving off nervous cords in a radiate manner, the cords, like those in Ascaris, being ganglionic. In Ascaris, if I am right, we have a ganglionic ring giving off several short and two long ganglionic nerve-bands. In the earth-worm, according to Claparède, we have a single ventral nerve-cord connected with a ganglionic ring. A step leads us to the condition seen in many Annelida—a single ventral nerve-cord with ganglionic enlargements, and the transition is easy to the whole series of Homogangliata.

If we compare the nervous system of Tetrastema, one of the Nemertids, with that of Ascaris, as I have described it, we shall be struck with their close similarity. In Tetrastema we have the lateral disposition of the main nerve-trunks, as in Ascaris; but they are no longer ganglionic bands. We have the œsophageal ring, but we have a pair of cephalic ganglionic enlargements added.

From all that I have said it will be apparent that the Nematoid worms stand in a clearly intermediate position between the Echinodermata and Annelida. The water vascular system with its vesicles remind one strongly of an Echinoderm; the pharynx, pharyngeal teeth, and segmented integument are clearly those of an Annelid, whilst the nervous system is more nearly like that of the earthworm than that of an Echinoderm. In Gordius I think there can be no doubt of this, where there is but a single ventral cord. I cannot agree with Dr. Bastian's view that the Nematoid worms are more nearly allied to Echinoderms than to Scolecida, although I must think much credit is due to him for having first pointed out strong affinities with the Echinodermata,—not stronger affinities, however, than those known to exist between Nemertids and Echinoderms.

The transition from the form seen in Ascaris to that of the anenterhelminths, especially Echinorhynchus, which has the lateral bands of Ascaris, seems to me to be indicated in the want of muscularity in the intestinal canal of Ascaris: and its remarkably modified form in Mermis, where it is entirely deficient, except in the anterior part of the body. Again, it is difficult to doubt the affinity between the Nemertida and the Nematoida.

I have little to say further on the anatomy of Ascaris, the reproductive organs have been so carefully described by Cloquet, Dr. Cobbold, and others. I will only add to their descriptions that the walls of the ovaries uteri and vagina, as well as of the testis and its duct, have a distinct muscular coat, upon which nerves and pseudo-hæmal vessels may be traced.

I have also a few facts to add concerning the development of the sexual elements.

The ova in the upper part of the ovaries somewhat resemble conical epithelial cells, and fill the whole ovarian tube, meeting in its centre or axis. They are white and very granular, and exhibit no distinct cell wall. I am doubtful about the nucleus, but believe I have detected it in several instances. These conical ova are more loosely packed in the lower six or eight inches of the ovarian tube, and begin to exhibit traces of division at their base. Lower still they have formed distinct buds, which become detached and form the yelks of the eggs. Meisner has observed a similar process of multiplication of ova in Gordius, and in some Lamellibranchiata. In the larger part of the genital tract, the so-called uteri, the ova become invested first in a strong yelk-capsule, and later in a beautiful sculptured chorion. The ova are deposited in strings, like those of Mermis, adhering by the chorions.

It has been stated that several months elapse, during which period the ova need to be submerged in water, before development proceeds in them to yelk segmentation; but I have specimens of eggs taken from a worm, which was placed in spirit as soon as it was voided, which exhibit several distinct stages of yelk segmentation.

The male sexual element exists in the upper part of the tubular testis in the form of minute distinctly nucleated caudate cells, attached to each other in a manner precisely similar to the budding ovarian cells; they do not exceed the $\frac{1}{2000}$th of an inch in their long diameter. Lower down in the testis they are polygonal from pressure, and have increased to $\frac{1}{1500}$th of an inch in diameter, still with conspicuously bright nuclei. The nuclei are replaced in a more advanced part of the testis by bright nuclear particles, of which there are from twenty to thirty in each cell. The remainder of the male sexual canal, which is very capacious in proportion to the testis, was entirely filled with similar cells, so that I am ignorant of any further changes which they may undergo.

The development of the ova as far as it is at present known has been described by Dr. Cobbold, I have, therefore, no more to add upon this head.

This concludes what I have to say on the structure of Ascaris. I have endeavoured as far as possible to avoid errors, and trust I have done somewhat to put the anatomy of the Scolecida on a more secure footing than it has hitherto possessed.

DESCRIPTION OF PLATES.

Plate LXXV.

Details of the Anatomy of Ascaris lumbricoides, &c.

Fig. 3.—A fibre of muscle from the body wall × 120.
„ 4.—The mouth seen in front. *a*, *a*, openings of water vessels × 30.
„ 5.—The pharynx laid open, showing the ridges in the interior, × 10.
„ 6.—Anterior extremity of one of the pharyngeal ridges × 40.
„ 7.—Anterior extremity of the worm laid open, showing, *b*, *b*, the water vascular, and *d*, *d*, the nervous bands, with their ramifications and connections, × 15.
„ 8.—The external opening of one of the water vessels × 120.
„ 9.—One of the lips × 30.
„ 10.—Cells from the upper portion of the testis × 750.
„ 11.—Cells from the middle portion of the testis × 500.
„ 12.—Cells from the duct of the testis × 500.

Plate LXXVI.

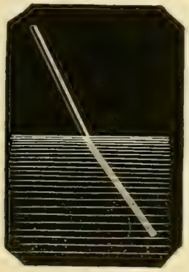
Details of Anatomy of Ascaris lumbricoides.

Fig. 1.—Half a transverse section, made about two inches from the head, × 20, showing, *a*, external layer of integument; *b*, cellular layer; *c*, muscular layer; *d*, lateral nerve-band; *e*, transverse water-vessels bearing vesicles (*appendices nouricières*); *f*, alimentary canal; *g*, dorsal water vascular trunk × 20.
„ 2.—A portion of the dorsal trunk, with transverse vessels, vesicles, and pseudo-hæmal vessels, detached from the other structures, × 25.
„ 3.—A portion of the testis, with pseudo-hæmal vessels ramifying upon it, × 60.
„ 4.—A transverse section of one of the lateral bands × 40.
„ 5.—A portion of one of the lateral bands, × 25.
„ 6.—Ganglion corpuscles from the same × 500.
„ 7.—A budding yelk from the lower part of the ovary × 200.
„ 8.—Three yelks from the upper part of the ovary × 200.
„ 9.—A budding yelk, from the lower extremity of the ovary, dividing into two, × 200.

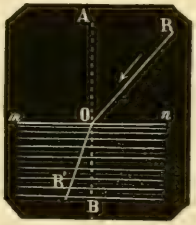
V.—*On the History, Refractions, Definition, and Powers of Immersion Lenses and New Refractometers.* By Royston-Pigott, M.A., M.D. Cantab., M.R.C.P., Fellow of the Cambridge Philosophical and the Royal Astronomical and Microscopical Societies of London, formerly Fellow of St. Peter's College, Cambridge.

(*Part I.*)

I.—History has handed down to us the labours of Claudius Ptolemy, the Father of Optics. His celebrated experiment of rendering a coin visible placed in an empty vessel by filling it with water, was perhaps the first scientific attempt at experimental refraction: thence he proceeded to measure the refraction of light passing from water into air, from air into glass, and lastly from water into glass. He sagaciously avoided the deviation from glass into air by taking care the rays should emerge perpendicularly to the surface of the glass.



Bent Oar in Water.



Refraction from Air into Water.

His very ingenious mode of settling these points, as it really involves the important principle of the modern immersion lens, may not be uninteresting to those unacquainted with the experiments. Observing the crooked image of an oar in water, and the curious apparent rising of the money as the water is poured into the vessel, he had a semi-cylinder of "pure glass" constructed, to which he adjusted a graduated circle, so that the diameter of the cylinder coincided precisely with the diameter of the graduated circle above mentioned, and also with the surface of the water. A small coloured body was placed at the centre of the circle; a second similarly fitted to one of the quadrants out of the water; a third slided on the lower part of the graduated circle immersed in the water. Observations were then made upon the angles of incidence and refraction of such a degree of accuracy as to excite our admiration after a lapse of nearly two thousand years. The determination of the conditions of refraction through a water lens was thus anciently effected sufficiently exact for all the practical purposes of modern theory.

Ptolemy observed that the refractions from water into glass were less than any he had previously observed. The following were some of the results he obtained:—

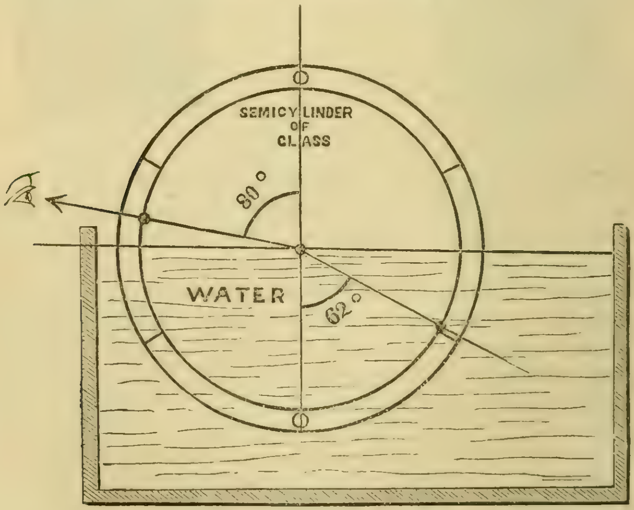
CLAUDIUS PTOLEMY'S TABLES.

Refractions from Glass into Air.			Refractions from Water into Glass.		
Angle of Incidence.	Angles of Refraction.		Angle of Incidence.	Angles of Refraction.	
°	°	′	°	°	′
0	0	0	0	0	0
20	13	30	20	18	30
60	34	30	60	49	30
80	42	0	80	62	0

The first Table on the left, for the 4 angles, gives the value of the refractive index for Ptolemy's "pure glass" at 1·48, 1·53, 1·47; the mean of which is 1·49, or nearly the refractive index for our common glass, and evidences the singular care Ptolemy must have taken in his experiments.

For the water refractions the values of the index of refraction between glass and water are (by the refractometer, to save the calculation by logarithms), for 60° and 80° in the Table, nearly 1·500 and 1·336, a surprising approximation to the true values by this primitive method.

PTOLEMY'S IMMERSION LENS.



Refraction from Water into Glass.

If the glass cylinder be removed, then the ray would be turned back by the surface of the water, and internally reflected, instead of being refracted.

This valuable experiment of Ptolemy I particularly beg to recommend to those opponents of immersion lenses who deny that by their means a much greater amount of rays radiating from a Canada-balsam mounted object ultimately reaches the eye of the observer than can possibly be effected by aërial or dry refraction in the ordinary way.*

In order to show the agreement between Ptolemy's Tables and those given in the papers by the writer "On the *Optical Advantages* of Immersion Lenses," it will be convenient to the reader to reproduce here the final Table of results from page 140, September Number, 1870.

TABLE V.

REFRACTIONS.	Ratio of Quantity of NASCENT Pencils Transmitted.	Aperture.	Angle of Total Reflexion.
Plate glass to air	1	83° 36'	41° 48'
" water	2¼	125 54	62 57
Flint glass to turpentine ..	3½	156 46	78 23
Refractions from Cover through Refracting Film.	Quantity of Defining Pencils.	Necessary Objective Aperture.	Limiting Angle of Transmission.

Tracing the history of immersion lenses, we may relate that Sir David Brewster employed lenses recently extracted from fish and other animals, as well as minute drops of castor oil, &c., formed into plano-convex lenses, and even a mixture of fluids of different dispersive and refractive powers, and indeed glass lenses immersed in fluids. This was about 1812.

In 1855 Professor Amici showed, at the Paris Exposition, a microscope acting upon the immersion principle, which "exhibited certain *striæ*† better than any of the instruments under examination" by the jurors in Class VIII. This superiority was produced by the introduction of water between the object and the object-glass; but as Professor Amici could not be considered as an exhibitor, the jury was not called upon to adjudicate to him a medal.‡

"The employment of fluids of various dispersive powers, says the writer in the 'Ency. Brit.,' gives the artist a wider range in his experimental researches." M. Achille Brachet, of Paris, has added, in Italian, M. Amici's own account of his invention.

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* See Mr. Wenham's Paper, Jan., 1871.

† The Amician Test for many years held a first rank as a test for the $\frac{1}{16}$th.

‡ "Mr. Andrew Ross intended to exhibit in Paris object-glasses of a still higher order than those we have mentioned, and would thus have found himself in competition with his eminent rivals, Messrs. Smith and Beck, and M. Nachet. He was prevented, however, by the pressure of business from exhibiting the new object-glass, and Messrs. Smith and Beck, who had no English rival, carried off the microscopic prize, by receiving a medal of the first class. Although the microscope of M. Nachet was not equal to that of the English artists, it had such a high degree of merit, that a medal of the same value was adjudged to him."—'Ency. Brit.,' art. "Microscope."

"These glasses were constructed on a new principle—that of immersion in water—and the most extraordinary point of the discovery was the very considerable distance preserved between the objective and the object under view."

"The series No. III. and No. VI. on this new principle renders the image more clear and distinct, and does not require any correction of the error which the common system introduces, owing to the different thickness of the covering glass. This advantage was obtained by immersing in a drop of distilled water the last lower surface of the series III. and VI."

TABLE OF AMICIAN ELEMENTS.

	No. III., Immersion.	No. VI., Immersion.
Thickness of covering glass ..	1^{mm} or 0·039″	0·010″
Inches, focal length	0·168″ or $\frac{1}{6}$	0·0685 or $\frac{1}{14}$ nearly
Aperture	70°	160°
Power, diameters	535	1310

"Professor Amici is of opinion that no object-glass hitherto constructed has a power as great as his Series No. VI., and considering the distance (about 0^{mm}·4, or 16 thousandths of an inch)* which is left between the object-glass and the object, with a large aperture of 160°, he thinks he has given proof of the superiority of the principle he has invented." ($\frac{1}{16}$ usually requires a "cover" only 4 thousandths thick 0·004″.) "Professor Amici tells us that his object-glass No. III. is composed of *five*, and No. VI. with *six* different refractive and dispersive substances." †

The English, with their stolid resistance to change and new ideas, had persisted, up to a very recent date, in ignoring the immersion principle. Indeed, none of our opticians have taken them up till compelled by the pressure of public opinion and ardent amateurs. The splendid abilities of Professor Amici, aided by his optical knowledge, perhaps surpassed herein even ANDREW ROSS, who had a lifetime to learn the Microscopic Art. There are even *now*, I believe, no dry objectives made of the $\frac{1}{14}$th of an inch focus, capable of viewing an object through a glass cover one-hundredth of an inch thick without adventitious aid.

Such, then, being the history of the immersion lens, our Continental friends, who crowded to Paris in 1855, became thoroughly acquainted with the superiority of the immersion principle, and have, accordingly, for many years been using them; whilst the English have been struggling with the formidable difficulties of correcting the glasses for aërial refractions; the film of air making enormous differences in the course of the excentrical pencils.*

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* A simple method of converting millimètres into inches, sufficiently near, is to multiply the *mm* by 4 and divide by 100, or cut off two ciphers.

† 'Ency. Brit.,' p. 783, Ed. 1857, art. "Microscope."

For whether we trace the rays downwards through a microscope to the object, or upwards from the object, no possible difference is made in the course of the ray so long as the conditions of the *media* remain the same.

A ray radiating from an illuminated particle immersed in the balsam and striking against the surface of the air above the cover, is totally reflected at an angle of about 42°; but substituting water for air at about 62°, as follows from Ptolemy's discoveries.

To use the words of one of the most distinguished writers on Optics in this country, to whom the writer submitted the principle of the water lens, advocated in these papers, a much greater number of rays from the object reaches the eye of the observer *viâ water*, than can possibly take place *viâ air*.

But not only do more than double the quantity of rays reach the eye through the objective and eye-piece, but they find their way more directly without such violent bending or abrupt refraction. This advantage is inestimable. For, as is well known, the aberration of a prism is a minimum only when the deviation is the least possible, and all lenses act on the principle of the prism.

The celebrated Huyghens invented his eye-piece entirely upon the principle of dividing the refractions as nearly as possible, so that each lens should bend the rays equally at a minimum deviation. Fortunately, and unexpectedly, the combination of the focal lengths of the lenses as 3 : 1, and separating the lenses by a distance equal to half their sum, also produced an excellent achromatism for parallel rays as it were by accident.

Messrs. Powell and Lealand have informed me that in using the greatest obliquity of illumination attainable by their peculiarly-

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* As it seems right in the eyes of some of the old lovers of dry objectives to dispute a number of things which they did not learn in their youth, it may not be out of place here to quote a principle, at which, doubtless, advanced optical students are quite *au fait*. The aberration of the direct ray refracted through a plate "of thickness (t), is equal to the thickness multiplied by unity divided by the refractive index.

$$i.e. = \frac{t}{\mu} \text{ and } \frac{1}{\mu} = \text{sine of angle of total reflexion.} \quad \text{(See p. 72.)}$$

For plate glass, it is therefore $\frac{2}{3}$ the thickness: for a film of water it is $\frac{3}{4}$ the thickness. But the dispersion is very different in the two cases: for oblique rays the aberrations for obliquity θ are respectively for thickness ($t = 1$)

$$\frac{2}{3} \text{ secant } \theta \text{ and } \frac{3}{4} \text{ sec. } \theta,$$

and enormous differences in the paths of the excentrical rays necessarily result." (See PARKINSON's 'Optics,' p. 79.)

mounted condenser, it was only when the immersion lens was used that the extremely oblique light from the object could enter the object-glass and pass to the eye; the object being mounted of course in Canada balsam. If, indeed, we plunge *the objective* into Canada balsam, to view an object immersed in it (as proposed by Mr. Wenham, page 18, line 7), in order to demonstrate the principle of the water lens by applying it without the water to prove its action with it (save the mark!), though they have very different refractive effects—why we can thus certainly *imagine* both effects are alike.*

It cannot be too clearly asserted that no dry objective, whatever be its aërial aperture, can receive within the substance of its front lens rays more oblique that 42°, nor with an immersion film of water more oblique than 62°. And, even then, the ray in both cases must strike the face-glass at an obliquity of nearly 90°. Yet Mr. Wenham says they are both alike! It would be unkind to suggest that Ptolemy's optics, nearly 2000 years old, have an advantage, as regards immersion lenses, over Mr. Wenham's. I commend the Ptolemy immersion lens to his especial cogitation.

II. *Refractions.*—In so interesting a question, the teachings of Nature herself should not be neglected. "The crystalline lens" is an "immersion" placed in two refracting fluids, possessing an aperture of above 100°. In fishes, the view under water is worth consideration. To the gaze of the fish the whole prospect of earth and sky is contracted within a cone of which the apex is the eye and the base a circle of about 96° angular aperture, beyond which many objects, as its fellow fish, appear double by total internal reflexion.† The angler on the bank of the river is seen in miniature. As objects approach the zenith, they enlarge rapidly. All the objects on the horizontal plane are crowded into a minute annular space.

If we hold a tumbler of water steadily above the level of the eye, any substance floating in the water may be seen internally reflected at the upper surface more brilliantly than any metallic surface can effect it, by reason of the total internal reflexion (Parkinson). This phenomenon beautifully explains the great loss of pencils radiant from the object, necessarily effected by the use of dry objectives with a Canada-balsam mounted object.

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* It is hardly necessary to inform the reader that the Plate LX. is drawn merely to illustrate the comparative angles of total internal reflexion in the case water *versus* air lenses, as also the deviations and aberrations of the *rays upon entering the first glass front of the objective.* Indeed, the curves of the upper lenses are quite unnecessary for my purpose; and the extremely minute scale of the refractions at the focus, when sufficiently enlarged, left no room in the plate to draw the lenses correctly. Mr. Wenham very properly points this out in his stricture on page 17, January, 1871. But this does not in the least affect the question of comparative refractions between the brilliant particle and the first surface of the objective: *the real point at issue*, in the case of dry and immersive *fronts.*

† *The aquariums* (now destroyed) exhibited very brightly the images of the gold and silver fish totally reflected by the upper surface of the water to the visitors at the Crystal Palace, who looked upwards through the side.

I believe that in this country the first notice of immersion lenses was by Mr. Mayall, jun., in a paper, Feb., 1868. Comparing the two methods, he says:—"The relative separating and defining power of the two systems have not received that attention which it deserves."

Mr. Mayall continues:—"It is not difficult to see that Amici's system of connecting the objective with the cover-glass by a film of water very much diminishes the reflexion which necessarily takes place in the incidence of the oblique light when the dry objective is employed. The limiting angle of refraction in water being about 48 degrees (*sic*), it follows that whatever is the degree of obliquity in the incident light on the object, the immersion objective never has to do with rays of greater obliquity than 48°. To this, in great measure, is due the greater clearness and precision of image obtained. Continental opticians and men of science have been aware of the merits of the immersion system during several years past; and to such purpose, that knowing how little attention it has received here, they do not scruple to say that the English no longer take the lead either as opticians or as microscopists."

This glaring error of 48° *most probably arose from the writer taking the angle of total internal reflexion between water and air instead of water and glass.*

In the 'Popular Science Review' (p. 330, 1868), the talented Editor remarks:—"Hartnack's immersion lenses appear to be getting into general use for work which does not require such high powers as the $\frac{1}{20}$th to $\frac{1}{25}$th objectives; but the subject of immersion lenses requires to be more fully worked out than it has been. We want experience of those who have employed both forms of object-glasses, and who can fairly and fully state the respective qualities of each."

In the 'Monthly Microscopical Journal,' Dr. Lawson observes (p. 217, vol. ii.):—

> "**Mr. Ross's New Immersion Lenses.**—Mr. Ross has just prepared a number of immersion lenses, which our readers will do well to examine. There is a decided improvement over the dry glass in definition, and there is vastly more light."

A recent writer in this Journal (Mr. Wenham) seems to follow in the wake of Mr. Mayall, for he states (p. 17, XXV.):—"The same optical law that limits the aperture of any object-glass to near 82° in a balsam-mounted object also determines the angle *in the lens* at which the rays diverge *after* being refracted from the plane surface of the front. This can never exceed 82° in a dry objective, nor can it be greater on the immersion system." *Verbum sat.*

It would seem that these writers must be unacquainted with the standard works on Optics, and so of course should not be expected to know that the angle of total internal reflexion or "*critical angle*," as it is called, is *dependent upon the refractive indices of both media through which the incident ray is passing.* Thus Dr. Parkinson, F.R.S., in his edition of Rev. W. Griffin's 'Optics':—

"*Definition.* — The angle whose sine is $\frac{1}{\mu}$, which the angle of incidence in the denser medium must not exceed, in order that refraction into a rarer medium may be possible, is called the *critical angle* of the media between which the refractive index is μ."

To find the mutual index of refraction between glass and water, ${}_{\text{glass}}\mu_{\text{water}}$ $\left(\text{or shorter } {}_{\text{G}}\mu_{\text{W}}\right)$ Dr. Parkinson gives the following example (p. 76, 'Optics'):—

"*Ex.*—From air into glass $\mu = \frac{3}{2}$: from air to water $\mu = \frac{4}{3}$. Hence from glass to water (denser to rarer medium)

$$ {}_{\text{G}}\mu_{\text{W}} = \frac{\text{index from air to water}}{\text{index from air to glass}} = \frac{8}{9}\text{"} \left(= \frac{4}{3} \div \frac{3}{2}\right) $$

in other words, the index of refraction for rays passing from water into glass is $\frac{9}{8}$, and the critical angle is that whose sine is $1 \div \frac{9}{8}$ or $\frac{8}{9}$, *i.e.* 62° 44'.* This nearly agrees with Ptolemy's 62°.

The distinction between the critical angles when *air* is not one of the refracting media is not given in general in *popular* works on Optics, although so well described by PTOLEMY.

Professor Haughton writes (p. 28, 'Optics'):—"The total

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* Ptolemy's value is 62°. Taking these values we have from the ordinary tables, since $\frac{8}{9} = 0{\cdot}88889$ nearly,

$$\log. 0{\cdot}88889 = \bar{1}{\cdot}94885 = \log. \sin. 62^\circ\, 44', \qquad \text{or } 62^\circ\, 44' \text{ instead of } 62^\circ.$$

The rough approximation $\frac{8}{9}$ gives nearly the same result as the decimal values of μ in the two cases, *viz.* 1·500 and 1·336, usually employed when the value becomes 62° 57'.

Again, for 80° the sine of angle of refraction $= \frac{8}{9} \sin. 80^\circ$

$$\frac{8}{9} \times 0{\cdot}9848078 = 0{\cdot}8753847 = \sin. 61^\circ{\cdot}5'$$

The refractive index of the glass used by Ptolemy, since that for water is known, was nearly that of plate or crown glass: as will be found by the formula

$$\mu = \frac{\sin. \phi}{\sin. \phi'}.$$

reflexion of light may also be illustrated by means of the following apparatus :—

"A glass chimney is fastened into the bottom of a rectangular vessel of glass, *a b c d*, which is filled with water, and a candle introduced into the chimney. The rays from the candle are seen to emerge from the water when the angle of incidence is less than the angle whose sine is $\frac{3}{4}$; but when the incidence exceeds this limit, the light is seen to be totally reflected, and none passes through the surface of the water." The angle whose sine is $\frac{3}{4}$ of course equals 48° 35′ nearly. Now will the reader suppose that a layer of oil of cassia is poured upon the surface of the water, then since

for cassia, μ the refractive index is $1 \cdot 635$
for water, μ „ „ $1 \cdot 336$

for the line E (of the spectrum of Fraunhofer) of mean refrangibility. Evidently it is quite impossible that the angle of total internal reflexion can be the same as before, when the ray, emerging from the water, strikes the under-surface of the cassia.

The value of $\frac{1}{\mu}$ is now $1 \cdot 336 \div 1 \cdot 635 = \cdot 8171$.

The angle whose sine is $\frac{1}{\mu} = 54° \, 48'$, which is a great increase on 48° 35′. For water and glass it is, as we have seen, about 62°.

The most important part of the subject is unquestionably—

III. *Definition.*—The writer cannot but regret that he has had the misfortune to incur the displeasure of Mr. Wenham in making the following statements in the interest of Microscopical Science :—

"Even in a good $\frac{1}{8}$th object-glass the spherical aberration does not exceed the 50,000th of an inch."

"The most difficult definition in the Podura is the substratum of beads glimmering through the membrane nearest the light. . . . The beading dispels the mist and haze always accompanying the spurious 'spines.'"

"That our old-fashioned glasses are wrong somewhere—in the correction of spherical aberration."

"That the battle of the glasses will have to be fought; and that the superiority of the immersion lens is an irrefragable proof of the corrections necessary." (Dec., 1869.)

"That in viewing the image of a flame in a minute globule the spurious disk is more than double the size of the true size of a perfect aplanatic image."

"That in the diameter of this spurious disk lies the whole pith of the objective corrections." (Dec., 1870.)

That the writer of the present article has described several appearances which Mr. Wenham has never yet acknowledged to have been able to see, who therefore denounces them as spurious!

It was then pointed out that double stars could be imaged to any degree of minuteness even 100,000th of an inch apart, upon a minute mercurial globule, in reply to Mr. Wenham's challenge.

Whereupon he roundly declares (January No., 1871):—

"I must protest against the positive manner in which Dr. Pigott still totally condemns the mercury globule. This self-confidence may be attributed to non-acquaintance with it."

Next, speaking of my double-star image test, he rather oddly admits:—

"That the separation of two points of light on the globule coming from two sources placed at a given interval is a test no one will question."

I am exceedingly glad to hear this. I was really afraid Mr. Wenham would annihilate this *experimentum crucis* after the solemn protest aforesaid. But Mr. W. is more generous still. He grants me the whole pith of the question in one full admission, which saves the writer from all further anxiety about Mr. W.'s opinion. He magnanimously asserts:—

"The greatest value of its indications *are* when it (the globule) is considerably within or without the focus. . . . That it is employed solely for the purpose of obtaining a distinct image is quite a mistake. . . . *No optician thinks of the possibility of a distinct image.*"

If the reader will turn to page 265, November Journal, 1870, he will find a statement of mine to the following effect:—

"The image of a flame reflected by the test globule is a round disk, much larger than it ought to be, as seen in all isolated brilliant points in the microscopical field. It is interesting to inquire at what size its image can be discovered." (*The reply is never!*) "And if such small globules are employed that the shape is gone, and if the real diameter of the spurious disk is a test of the correction of the glasses, what becomes of the boasted accuracy of the globule test, in which the size of the spurious disk is totally neglected? No one ever thinks, in testing microscopes, at all about this spurious appearance. In telescopic testing, the most essential part of the ordeal is the diameter of the spurious disk."

Mr. W. now declares that the globule test is used principally for correcting the combinations of the lenses in detail, beginning with the back set first, in which excessive aberrations are purposely introduced, and therefore not "to correct the least fault."

So that its employment to test the real focal point and the size of the spurious disk is, after all, exactly what has been stated. The fact is, for the deep objectives, anything but a wild attempt to get a *mercurial image* from the globule placed immediately under deep objectives *is impossible.*

I cannot properly conclude this article without some allusion to the edifying and philosophical personalities which grace Mr. W.'s papers. I can only ask the readers of this Journal to decide whether he has forfeited the pledge he gave to carry on the controversy "in a fair spirit, being willing to receive or give any information that may tend to elucidate truth" (p. 301, June, 1870). [The "information" would surely have been quite as valuable without the ornate *argumentum ad hominem* so liberally employed.] His chief reason for commencing the controversy being, as he states in the same page, "the slur that is cast upon the object-glasses of our best makers by the assertion, that in the best glasses there is a residuary aberration which obscures the clear definition under a power of 1000." It is fair to compare this with the new admission. Speaking of my double-star test, he says (January, 1871, p. 22), "*A bad glass will blur them together, and a good one will separate them more or less distinctly.*" Do any of the old-fashioned glasses or Mr. Wenham's best objectives blur them?

"As no optician thinks of the possibility of a distinct image" whilst using the mercurial globule at present, I entertain a lively hope that the time is not far distant when they will use the *image tests* such as I have elsewhere described, as an exquisite means of detecting unsuspected residuary errors. Mr. Wenham, however, seemed at one time well aware of the many difficulties attending the construction of object-glasses, as the following passage fully demonstrates:—

"I am of opinion the case is different with a microscope object-glass, wherein, with the highest powers, every *trifling error* is enormously magnified" (p. 228, V.).

On the other hand he declares, "the effect of the water and covering glass is precisely the same in its corrective action as additional thickness thrown on the front lens!!!" The same very singular idea runs through the last effusion of Mr. Wenham.

It is, however, tolerably well known among physicists that the effects of irrationality on chromatic dispersion vary very considerably according as glass or water is the refracting medium,* and the thicknesses under use.

I feel quite certain that the best makers of English immersion lenses will hardly endorse either this adventurous statement, or another, to the effect that "from an ⅛th upwards perfect correction can be obtained from a single front."

I am greatly impressed with the admission that the chief use of the globule star is to examine the intended errors of the parts, and not the requisite performance of the complete objective.

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* See art. "Light," 'Ency. Met.'

I have submitted the very finest immersion objectives now known to the image test, and, as might reasonably be supposed with all human workmanship, I find the residuary error small, but still appreciable. The greater the angular aperture, the greater rises the almost insurmountable difficulty of compelling the oblique pencils to converge to exactly the same focal point as the more central rays (within a ring much less than the 50,000th of an inch). Still, the best glasses are a marvel of skill, worthy of the science of the nineteenth century: the least circle of aberration being so exceedingly small. No first-rate optician claims absolute perfection.

I think it right to add to this paper that I possess *an inch objective which displays the beading of the Macrotoma podura*, and which I had the honour of exhibiting to Dr. Lawson on the 14th instant (with about 600 diameters)—in order to avoid imaginative effects—before he had seen them with the half-inch of Wray, and the $\frac{1}{8}$th, and $\frac{1}{16}$th immersion of Powell and Lealand.

I am unwilling to retire from this controversy without recording an example of Mr. Wenham's "fair spirit"—or rather spirited—attacks upon these papers (and highly suggestive):—

"Our thanks are due to Dr. Pigott for his laudable attempts to advance the microscope object-glass; but as he appears to stand forward as the pioneer of a new era in their construction, ignoring all glasses made before the publication of his first essay as 'old-fashioned,' such dictatorship naturally challenges inquiry as to the merits of his investigations, and whether they have in any way contributed to the end in view" (p. 16, 1871).

Whether object-glasses are perfect, as Mr. Wenham guarantees them,—whether any error can be detected, and whether the glasses of 1871 are finer than they were in 1869, I leave to the decision of competent adjudicators, free from prejudice, and free from compromised opinions. If my declaration of the detection of the 50,000th of an inch aberration in the best glasses is thought so prodigious, I must exercise greater circumspection so as not to offend susceptibilities as unwonted in delicacy as conspicuous in refinement. I now consider the controversy has extended long enough to tire the patience of our Fellows, whilst the tone adopted creates an imperative necessity for bidding it adieu in pages devoted to the 'Transactions of the Royal Microscopical Society.'

As there is an emulation of no common kind between American and English microscopists, the following passage from the 'American Cyclopædia,' vol. xi., "Microscope," 1861, is not without interest. The article claims superiority for American glasses. Speaking of the *Amphipleura pellucida*, 130,000 lines to the inch, it says:—

"Mr. Sollitt and Mr. Lobb both claim to have resolved it from experiments made with the Nobert test-plate, and from the most careful examination with the best English and American objectives, the most experienced American microscopists believe there is some mistake." This object has been so frequently seen in this country that English microscopists would be glad to hear whether our American friends still disbelieve in its resolution, as this fact would be a strong argument against this supposed "American superiority."

It is a highly curious psychological phenomenon, that whenever some new advanced microscopical facts are discovered, old-fashioned observers honestly believe their advocates to be the victims of delusion, spurious appearances, ghosts, or mistakes.

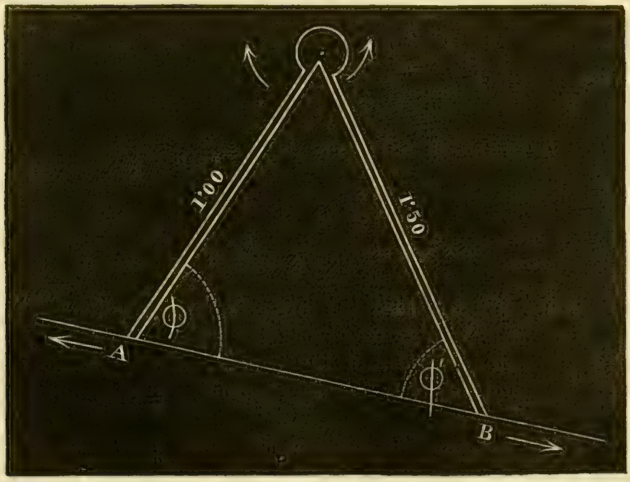
IV. *Description of two Refractometers,—the one Mechanical, the other Logarithmic.*

Experiencing the tedious work of constructing tables of deviation for rays of light passing through successive media of known refractive power, it occurred to me that an instrument could be contrived which should mechanically give the angles of refraction for any angle of incidence between any known media.

Let a triangle be constructed none of whose angles are greater than 90°, it is well known that the sides are in the exact proportion of the sines of the opposite angles. Thus if A B C be the triangle, a, b, c, the sides in inches,

$$a : b :: \sin. A : \sin. B.$$

Now, suppose that a, b, represent the refractive indices of two substances, then the sines of the angles of refraction and incidence are as a and b. From this it follows, that if a triangle be constructed whose sides represent the refractive indices of two given



substances, the angles between those sides and the base exactly represent the angles of refraction and incidence between the two substances. The diagram represents the angles (ϕ and ϕ') of incidence and refraction, when $\mu = 1\cdot500$; and for air, $\mu = 1\cdot00$ nearly. When the angle ϕ at A becomes 90°, the angle B or ϕ' represents the angle of total internal reflexion 42° 48′, and so for any other example.

The logarithmic refractometer consists of a slide rule, the upper lines of which represent the refractive indices; and the lower lines and divisions give any angles from 90° down to 30′.

A	·	·	1	2	3		
B			1	2	3	90°	sines.
						90°	sines.

The angles of total internal reflexion are given at sight opposite 90° by merely setting the refractive indices of the two media proposed opposite to each other.

Further examples.—Find the angles of total internal reflexion for the following substances, the ray passing into them from air, refractive index taken $= 1\cdot000$.

The refractive index is placed on B under $1\cdot000$, on A under 90°. The answers must then be interpreted.

Substance.	Tabasheer.	Oil of Turpentine.	Canada Balsam.	Spinelle.	Blende.
Refractive index ..	1·111	1·478	1·549	1·764	2·260
Answers	64°	42½°	40°	34½°	26¼°

Find the aberration *into air* of a "*plate*" of each of these substances as given in the note, page 69, $t \times \frac{1}{\mu}$, *i. e.* $t \div \mu$.

Sines	Sin. 64°	42½°	40°	34½°	26¼°
Values	0·90	0·675	0·643	0·565	0·44

From which it appears that the lower is the refractive index of a film or cover, the greater effect has the thickness of it upon the primary aberration *into air* of the pencil radiating from the illuminated particle under view; another reason for preferring the water film.

The above example is simply worked by placing 10 B or 90° over the angle whose sine is desired, and reading off the numbers under 1 on A: as $1 \div \mu =$ sine of the angle of total reflexion in each case.

On the lines A, B, innumerable questions of refraction and internal reflexion are answered on inspection.

It gives at sight also in most cases to three places of decimals the sines of angles, thus:—

Sin. 21½° = ·365.* Sin. 10° 20′ = 0·179. Sin. 5° 5′ = 0·0885.

Again, if a ray of light passes from water into diamond, *water* $\mu = 1{\cdot}336$ *diamond* $\mu = 2{\cdot}47$, by placing these indices over each other, at sight we see the following angles:—

Incidence	9°	..	18¾°	..	26½°	..	38°	..	57°	..	90°
Refraction	4° 53′	..	10°	..	14°	..	20°	..	27°	..	33°

The last, 33°, being the angle of total internal reflexion, and subtracting one from the other, the deviations at given angles of incidence are at once shown on the rule. The rule thus shows these quantities:—

A	(Water) 1·336						
B	(Diamond) 2·47						
	9°	18¾°	26½°	38°	57°	90°	Sines.
	4° 53′	10°	14°	20°	27°	33°	Sines.

VI.—*On Microscopical Appliances.* By Dr. Royston-Pigott.

No. I.

On a Useful Form of Dynameter Micrometer—the Kratometer.

It some time since occurred to the writer, that an instrument could be so contrived in the form of an eye-piece which should serve some very useful purposes of measurement, such as, (*without the constant use of the rule of three*)

(1) To give, at sight, the magnifying power of any objective for any length of tube in use at the time of observation.

(2) To ascertain the standard focal length of object-glasses of all kinds, whether conventional or solar (for parallel rays).

(3) To ascertain the focal length of any given single lens required to be tested.

(4) To deduce the actual focal length of eye-pieces.

(5) To measure objects in the field of view under any power or length of tube.

(I) In the present number of the Journal I shall only embrace the opportunity of describing the mode of fitting up the instrument, and ascertaining magnifying power. It is a negative eye-piece, the lenses of which are so arranged as to magnify just five times under the condition of the eye-lens magnifying ten times. Within the focus of the eye-lens is placed a circular micrometer scale ruled to $\frac{1}{200}$th of an inch. Upon placing another micrometer ruled to $\frac{1}{100}$ths and $\frac{1}{1000}$ths upon the stage, it is plain the divisions of the latter are exactly five times smaller than of the former. Hence, since the eye-piece magnifies the image to the eye five times, it follows that whatever power be applied by means of the objective and length of tube, the actual reading of the stage $\frac{1}{1000}$th by the eye-piece will give the actual magnifying power under use. This method was employed as well as the camera lucida in estimating the magnifying advantages gained by the Searchers alluded to in the December article. The extreme simplicity of the instrument is its greatest recommendation; although its *modus operandi* is not so very clear at first sight: but perhaps the following statement may be of use towards its explanation.

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* The rule is made by Cary, 181, Strand.

If, for instance, we view a thousandth on the stage with a "quarter," and the Kratometer eye-piece, we shall see, without using the draw-tube, that a thousandth is covered (say) by 35·5 divisions of the eye-piece; therefore

$$\text{Power of "Quarter"} = 355 = \text{P}.$$

If we draw out the tube several inches we may see that

$$\text{P} = 520.$$

NEW BOOKS, WITH SHORT NOTICES.

We greatly regret that owing to the pressure of articles on our space we are compelled to allow to stand over notices of the following works:—

'A Report on the Microscopic Objects found in Cholera Investigations.' By T. R. Lewis. Calcutta.

'The Natural History of the British Diatomaceæ.' By A. Scott Donkin, M.D. London: Van Voorst.

And 'Microscopic Objects Figured and Described.' By J. H. Martin. London: Van Voorst.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Abdominal Antennæ of Insects are Sense Organs.—The 'American Naturalist' for December publishes a note on the above subject, by Mr. A. S. Packard, jun., which is of considerable interest. Referring to Dr. Anton Dohrn's note on the subject in the 'Journal of the Entomological Society of Stettin, 1869,' he points out that he gave notice of the above structures as early as 1866, in the 'Proceedings of the Boston Society of Natural History.' He says:—I have been able to detect sense organs (probably endowed with the sense of smell) in the short, stout-jointed, anal stylets of the Cockroach (*Periplaneta Americana*), beautifully mounted by Mr. E. Bicknell. I have recently, after reading Dr. Dohrn's note, observed the sense organs and counted about ninety minute orifices on each stylet, which are probably smelling or auditory organs, such as are described by Hicks. Mr. Bicknell has counted more carefully than I did the exact number of these pits, and made out ninety-five on one stylet and one hundred and two on the other, adding, "there were none on the underside of their appendages that I could see." They were much larger and much more numerous than similar orifices in the antennæ of the same insect, and were situated in single rows on the upper side of each joint of the stylets. During the breeding season a peculiar odour is perhaps emitted by the female, as in vertebrate animals, and it is probable that these caudal appendages are endowed with the sense of smell, rather than of hearing, that the male may smell its way to its partner. This is an argument that the broadly pectinated antennæ of many moths are endowed rather with the sense of smelling than hearing, to enable the males to smell out the females. I have observed the same organs in the lamella of the antennæ of the carrion beetles, which undoubtedly depend more on the sense of smell than that of touch or hearing to find stinking carcasses in which to place their eggs.

Motion of Microscopic Granules.—In a late number of the 'Boston Journal of Chemistry,' a writer who signs himself C. S. makes some useful comments on Mr. Wake's views on spontaneous generation. The author thinks it is not credible that organic germs could have retained vitality after exposure to the heat which Mr. Wake applied to the milk; it is not credible that eggs of infusoria like *kolpoda* could have been in the milk, if that had not been exposed to the air; and it is to be presumed that Mr. W. would not have experimented on milk that had not been protected. Is there any other source of error? Mr. W. says he put the burnt "residue" in a bottle half full of distilled water—size of bottle not given. Now the author has shown in the above-mentioned journal that he had found it impossible to procure distilled water perfectly pure. What germs were in the distilled water the experimenter did not know. Like hundreds of others, he takes it for granted that there can be none, and then attributes everything he discovers to the matter experimented upon, and not to the medium used. But what did he find beside the kolpoda-like animal? Something like *amœbæ*, an object about the nature of which very little is known, and a "mass of organic matter" which "had attached to it great numbers of small infusoria, which, by continual jerking movements, endeavoured to free themselves." Here is where he believes the great mistake of eminent microscopists has often been made, probably in part owing to inferior instruments. They have assumed that a body with a "jerking movement," or any movement, must be an infusorium, or a germ, or some organic being. This is the important point now for the study of microscopists. In August of last year, Mr. D. S. Holman, of Philadelphia, brought to him a slide which he wished examined. It was placed under the microscope with a *good* objective, and he saw the field filled with an immense number of minute spheres, all in movement, as lively as a party in a ball-room. He at once saw that they were what have been called monads by some writers, germs by others, and "bioplasm" by Beale. Mr. Holman then informed him that what he saw was albumen—white of eggs—coagulated by carbolic acid; that it was prepared, mounted, and completely sealed up from access of air, in July, 1869; and that the lively movement had been going on constantly (at least it was always seen when looked at) ever since. A few days since, with the view of verifying Mr. Holman's experiment, he prepared some white of egg himself, and has obtained the same results. The whole field of the microscope is filled with minute granules, particles, monads, or germs, all dancing together. Now here we have matter in which it is impossible (if human reason can pronounce an opinion on the subject) that any animal or vegetable life can exist, and yet in which there is present that one evidence of life, motion; and this motion has continued in the first-mentioned slide unchanged for fifteen months. Nor is this all. He has prepared slides of inorganic matter,—*e. g.* minute particles of chalk and china clay, suspended in a solution of glycerine in alcohol (can any life be sustained in such a medium?)—and these not only present the same movements, but an expert eye cannot distinguish a particle of organic from one of inorganic matter, with the same magnifying power. This movement of particles of matter in a fluid is no new thing; it has been known for years, and the text-books on the microscope all caution novices against being deceived by it; yet he has good reason for thinking that many who are experts have been so deceived. For a continued exhibition of the phenomenon, he believes it to be essential that the matter should be so near the specific gravity of the fluid as to remain in suspension. In the case of chalk and of clay, the particles in time settle in contact with the glass, and then are motionless until dislodged by jarring them.

Structure of Crustacean Shells.—Mr. Edward Parfitt has communicated to the Devonshire Association a paper on the above subject, which is extremely valuable, and ought really to have been published in this Journal. The subject, which is too large to admit of our giving a fair abstract, is in great measure a summary of the views of other writers, although there are many perfectly original matters mentioned. This author says that he has not seen it noticed by any writer that the pigment layer (he speaks of it now as if it was but one, instead of being made up of a series of layers) is still supplied with pigmentum after the shell appears to be complete, but such seems to be the case, for he was fortunate enough, when removing the inside membrane, or chitonous layer, to observe the pigment vessels *in situ.* This inner membrane is wrinkled into folds on the side next the shell, and these folds are collected round an areolated centre, having a larger orifice in the centre of the areolæ. This is one of the openings or communications between the animal and the corium; around these centres and on the folds are placed from four to seven pigment vessels, some of which were full and others empty. The full ones were distended and filled with minute points of red colouring matter. The vessels are composed of two membranes, an outer and an inner; the inner one does not entirely fill the outer one, as a considerable space occurs between the two. These vessels have necks sufficiently long to penetrate through the shell, and reach the pigment layer. He could not discover the secretory glands or cells that might be attached to those vessels, even with the highest magnifying power at his command. The empty vessels somewhat collapse after the pigment is discharged, as he observed several of them quite empty. The author also describes in *Cancer pagurus*, on the upper surface of the inner membrane, or on the last layer that has been added to the shell, if a thin horizontal section be made, and a strong light be forced through it, a number of peculiar stellate bodies. The rays are set round about a convex disk perforated with one or two holes. These rays have somewhat the appearance of crystalline masses, and are principally composed of calcareous matter secreted by the crab. The centres of these rays correspond with, and are the representatives of the little bosses on the surface of the shell; and through the orifices in the centre, appear to be the chief if not the only means for the deposit of the earthy matter. It is these convex disks which give the curvature to the various membranes as they are secreted and fixed; in fact the membranes are moulded over these, and they consequently take their form. After a time these rays touch each other, but, so far as he can see, they never overlap, but regular lines of demarcation are formed; and, curious enough, these lines form hexagons, in most instances as true as he has sketched them, and the lines quite as sharp; in others they are not so regular, as a smaller one sometimes intervenes. In time the interstices between the rays get filled up with calcareous matter, and the rays, except their extremities, become almost obliterated, and the shell shows only a homogeneous mass. These rays, he presumes, are deposited more for strengthening the membranes than for anything else, as, for instance, in the carapace of the shrimp, and in the large claw of *Pagurus Bernhardus*, they are scattered over the membranes irregularly, and do not appear to be confined to the papillæ processes so strictly as in the genera *Cancer* and *Carcinas*. The author's plates illustrate these views fully, and lend an additional interest to the work.

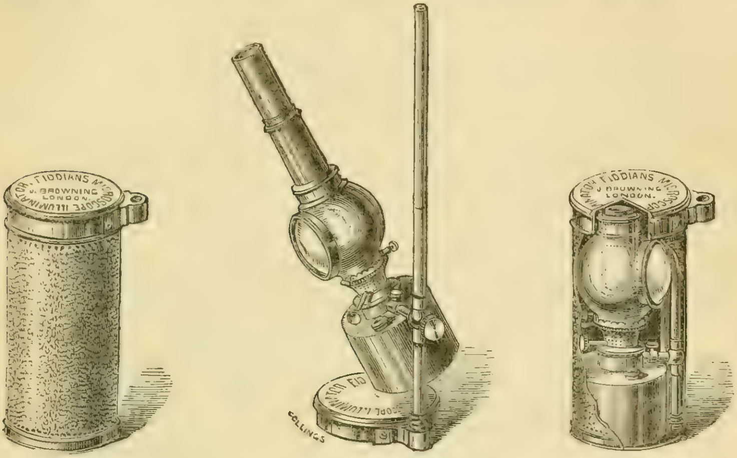
NOTES AND MEMORANDA.

Blood Stains.—We have to thank our contemporary, the American 'Medical Investigator,' for quoting some remarks which appeared in our pages on the above subject both from the American investigator Dr. Richardson, and from our English *savan* Professor Gulliver. We merely mention it because the note is signed J., and we are as yet unconscious from this to whom we are indebted.

The American Journal of Microscopy.—From the advertisement of this journal, which is published in so large and flourishing a town as Chicago, we expected much. We have, however, received a copy of the magazine, and we cannot say very much in its favour. It is a somewhat badly-printed journal of inconvenient 4to size, and is full of a series of articles of a far lower type than is to be found in our most familiar natural history journal, 'Science Gossip,' while its illustrations cannot compare for a moment with those of our English contemporaries. We hope that it is only the first number which will have this appearance, and that as it goes on it will get better. As it is, it is really worthless to English microscopists.

Browning's Microscope Lamp.—As illustrated on the opposite page, we have been furnished by Mr. Browning with a new microscopic lamp, which from our examination seems not only to be the best thing of the kind made, but appears as though improvement upon it were impossible. As shown in the engravings, it packs into a marvellously small case, 6 inches by 3 inches, and is, therefore, most convenient for carriage, which will be a great benefit for those of our Fellows who are in the habit of taking their microscopes with them to their friends. It was originally designed by Mr. Fiddian, and bears his name upon it. The following description will make it clearer to the reader:—The metallic chimney being telescopic occupies a very small compass; the condenser fits into the cell in front, which is also provided with plain and tinted glass for correcting the colour of the flame. The reservoir is of brass, and will contain sufficient petroline for six hours' consumption. The entire lamp fitting into the case from the top, escape of the oil is prevented.

In trimming the lamp care should be taken that the wick is perfectly dry, and the petroline of good quality; also that none of the oil gets upon the metallic chimney or reservoir, or a bad smell will be given off until the oil is burnt away. In using the lamp it will be found convenient to slightly incline it, so as to bring the broad surface of the flame more parallel with the surface of the mirror of the microscope. When it is necessary to re-line the chimney, screw off the sliding portion, wash out the old lining, and re-coat it with superfine plaster of Paris. When dry it will be found ready for use—a few minutes will be found sufficient to do this.



Photograph of our late President, Mr. Reade.—An admirable photograph of our late President has been executed by Messrs. Readell, of 49, Wigmore Street. It is sold at an extremely low price, and as it has been taken comparatively recently, it is a good representation of the dear old man.

The Microscope at the next Exhibition.—A meeting was some time since held at the Society of Arts to determine the various classes of objects which it is desirable should be admitted for exhibition. It was resolved to recommend to the commissioners that the following classes of specimens, if prepared and arranged from an educational point of view in the widest sense, should be received :—Prepared skins of birds and animals (prepared by new processes), skeletons, anatomical figures, microscopical objects and preparations, mineral and geological series and specimens illustrative of the geology of various districts, fossils, maps (physical), Ward's cases and aquaria, botanical specimens, and educational works on natural history.

Nobert's Nineteenth Band.—We greatly regret that we are compelled, for the third time, owing to press of matter, to omit Mr. Charles Stodder's interesting paper on Nobert's Nineteenth Band. We hope, in our next, to bring it at last, and after so long a delay, before our readers.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *January* 11, 1871.

James Glaisher, Esq., F.R.S., in the chair.

The minutes of last meeting were read and confirmed.

A list of donations to the library and cabinet was read, and the thanks of the meeting given to the respective donors.

The Secretaries exhibited two slides for opaque objects, furnished with neat and easily-removed thin brass covers to protect objects. These useful slides, invented by Mr. Aylward, were presented by Mr. Jackson, Hon. Sec. of the Lower Mosley Street Schools' Natural History Society of Manchester.

It was also announced that a specimen of diatomaceous earth and shells from Caracas had been sent by Mr. A. Ernst. In announcing this present, the Secretary said that before next meeting he would endeavour to get the shells uncovered and the earth examined.

A vote of thanks was proposed to each of the donors.

The Chairman then announced that Dr. Lawson, the Editor of the Journal, would bring forward the motion with reference to the alteration of the day of meeting, of which notice had already been given.

Dr. Lawson said that great difficulty had arisen in the publication of the Journal, owing to the fact that the second Wednesday in the month sometimes fell on so late a date as effectually to prevent the issue of the Journal on the 26th or 27th of the month, later than which no Journal could be fairly and successfully published. If, therefore, it should suit the convenience of the Fellows to meet at an earlier date, much trouble would be saved in the preparation of the matter for publication, and the Journal could be more efficiently managed. He accordingly proposed that the future meetings of the Society be held on the first, instead of the second, Wednesday in each month.

Mr. Slack seconded the motion. Upon inquiry he had found that the alteration would cause the Society's meetings to clash with those of other societies much less than at present, and would frequently enable the Fellows of the Geological Society to be present, who could not now attend. The Council had looked at all the circumstances of the case, and had come to the conclusion that it would be advisable to support Dr. Lawson's proposition.

The Chairman having put the question that the proposed alteration be made, and that it commence from March next, he declared it to be carried, with only one dissentient vote.

The Chairman then read the house list to be proposed at the next meeting.

The Chairman read a paper left by Mr. W. H. Ince, proposing the appointment of two new Secretaries in place of Messrs. Slack and Hogg, which did not find a single supporter, and fell to the ground.

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* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. M. M. J.

Mr. T. Charters White read a short communication "On Phycocyan."

The thanks of the meeting were given to Mr. White.

Mr. Slack read a paper "On the employment of Colloid Silica in the preparation of Crystals for the Polariscope."

The thanks of the meeting were presented to Mr. Slack.

Mr. B. T. Lowne read a paper "On the Anatomy of Ascaris lumbricoides."

The Chairman at the conclusion of the paper testified his sense of the great care, minute observation, and accurate delineation displayed by Mr. Lowne in his communication.

Dr. Braithwaite congratulated the Society on the inauguration of the new year by the remarkable paper just read, which he considered one of the most valuable ever presented to the Society.

Dr. Lawson concurred in these remarks, and observed that so many new facts and views had been recorded by the author that it would be utterly impossible to comment on them then. He thought the author merited the best thanks of the Society for his efforts.

The Chairman, in proposing a vote of thanks to Mr. Lowne, expressed the hope that he would continue his investigations, and favour the Society at some future period with the results he might obtain.'

Mr. Hogg suggested that any Fellow who should meet with the entozoon described should send it to Mr. Lowne. The subject was an important one in a medical point of view, and much light might be thrown upon it by inquiries such as those Mr. Lowne was conducting. In medical practice it not unfrequently happened that cases similar to that mentioned by him were brought under the notice of the practitioner, in which Ascarides formed a prominent feature in disease. He (Mr. Hogg) had had children repeatedly brought before him in order that a horrid squinting might be corrected, in whom there were certain indications that Ascarides was the cause of the disease; and he had not long ago seen a child, between five or six years of age, from whose mouth Ascarides had been observed to crawl, thus confirming the statement made by Mr. Lowne. He believed that Mr. Lowne had corrected some erroneous impressions which were current in regard to these animals. Regarding the investigations so ably conducted by Mr. Lowne, he (Mr. Hogg) could not help wishing that in this country, as in some Continental nations, men of such original powers of research were enabled, through provision made by the Government for the purpose, to carry on their labours, undisturbed by the harassing cares of an arduous profession.

The thanks of the meeting were then given to Mr. Lowne.

In acknowledging the vote, Mr. Lowne took occasion to remark on one point on which he had touched in his paper, that as yet we did not know how the embryonic forms of Ascaris got into the intestinal canal. He had also observed, contrary to the experience of some writers on the subject, that the yelk segmentation was to be seen in the egg while still in the body of the animal. According to Dr. Cobbold and some German authors, this never took place until after the egg had been soaked for a long time in water. He (Mr. Lowne) had found the yelk segmentation without subjecting the eggs to this process. He had not seen any embryos in the egg, not having had sufficient opportunity for developing this point. He hoped, however, to be able to work it out in future observations.

Mr. Hogg said he had been struck by the comparisons made in reference to these earth-worms; and it had brought to his mind Mr. Lee's investigations on the land-leech. He should like specimens of this animal to be placed in Mr. Lowne's hands. In this leech he (Mr. Hogg) had observed that the mouth and nervous system corresponded very closely to that described by Mr. Lowne in Ascarides.

Mr. Lee said he had a few specimens of the leech referred to by Mr. Hogg (*Trocheta subviridis*), which he should be happy to place at Mr. Lowne's disposal.

The President stated that as a new list of the Fellows would be printed as soon as possible after the Anniversary Meeting, all changes of residence should be immediately communicated to Mr. Walter W. Reeves, the Assistant-Secretary.

The meeting was then adjourned to the 8th of February (the Anniversary), when the Officers and Council for the ensuing year will be elected.

Donations to the Library and Cabinet, from November 9th, 1870, to January 11th, 1871:—

	From
Land and Water. Weekly	*Editor.*
Nature. Weekly	*Editor.*
Society of Arts Journal. Weekly	*Society.*
Athenæum. Weekly	*W. W. R.*
Royal Society's Catalogue of Scientific Papers. Vol. IV.	*Royal Society.*
Canadian Journal. No. 6.	
On the Size of the Red Corpuscles of the Blood of Moschus, &c. By George Gulliver, F.R.S.	*Author.*
Sketches to a Scale of the Auditory Organs of certain common Molluscs. By George Gulliver, F.R.S. ..	*Author.*
On the Taxonomic Characters afforded by the Muscular Sheath of the Œsophagus as regards Sauropsida, &c. By George Gulliver, F.R.S.	*Author.*
The Student. No. 5. New Series	*Publisher.*
The Popular Science Review. No. 38	*Publisher.*
Science Gossip. Vols. I., II., and III.	*Walter W. Reeves.*
American Journal of Microscopy. No. 1	*Publisher.*
A Set of Photographs of Insect Preparations. By Dr. Thos. Hallifax, of Brighton	*Dr. Hallifax.*
A Microscope and Set of Powers including a $\frac{1}{30}$ by Dolland	*The late President.*
The Wonders of the Microscope; or an Explanation of the Wisdom of the Creator. Anon., 1811	*F. H. Ward, Esq.*
On the Discrimination of Fibres in Mixed Fabrics. By John Spiller, F.C.S.	*W. T. Suffolk, Esq.*
Half-dozen Slides, mounted in brass, by Cuthbert, of Bat and Mouse Hair, Lepisma and Podura Scales, &c...	*The late President.*
Half-dozen Slides of Insect Preparations, mounted by W. H. Walmsley, of Philadelphia, U.S.	*W. H. Walmsley.*
Two Specimens of Mr. Aylward's New Slides for Mounting Opaque Objects	*W. Jackson, Esq.*

Lord Lindsay was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.

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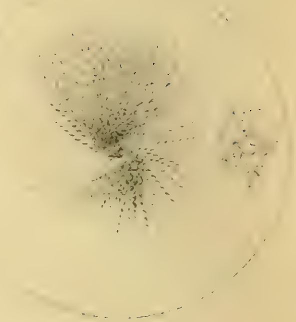
Copper 3



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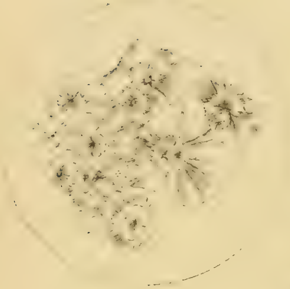

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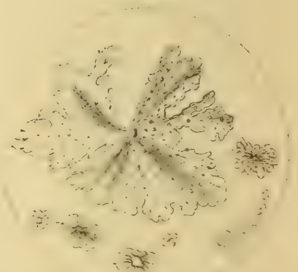

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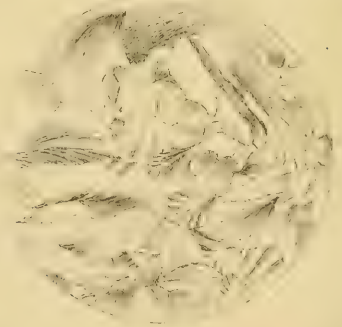

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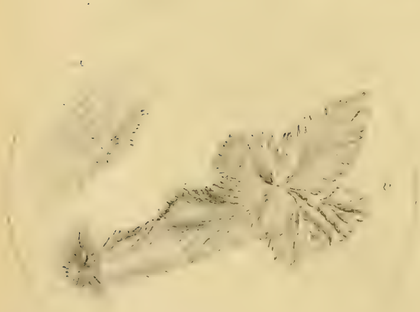

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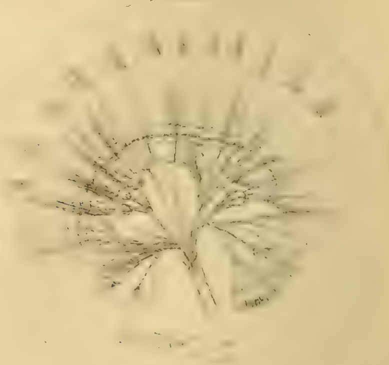

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THE

MONTHLY MICROSCOPICAL JOURNAL.

MARCH 1, 1871.

I.—ANNUAL ADDRESS.*

In surveying the work of last session the Society may find much reason for congratulation, from the variety and importance of the papers brought before it. In a remarkable series of contributions Dr. Pigott has made us acquainted with the nature and extent of uncorrected aberrations in our best object-glasses. He has explained more fully than had been done before, the advantages to be gained by the immersion system; he has called attention to sources of fallacy arising from what he terms *ειδωλα* (*eidôla*), found above and below the true focus; he has devised new tests, and introduced a new mode of correcting aberration by the use of his "Aplanatic Searcher." The novelty of this invention consists in searching the axis for best focal points at places intermediate between a fixed eyepiece and the objective, and applying new and variable corrections there. The aplanatic searcher also permits the use of a low eyepiece of 3 inches focus (the usual A being 2½). Further than this, it is said to increase focal depth, raise the power of the objective, improve the definition of parts seen in perspective, increase the working distance of the objective from the object, and lessen aberration so that delicate structures invisible with ordinary means can be perceived.

Those who have seen most of Dr. Pigott's experiments entertain the highest opinion of his investigations and suggestions; but the Searcher has not been successful in the hands of all who have tried it, and certainly requires considerable practice and skill for its best use.

Mr. Wenham, while controverting many of Dr. Pigott's opinions, has given much important information, not hitherto made public, on the construction of lenses; and having regard to his remarkable skill, and to the success of his past efforts, we may look forward with hope to his rendering still further service in bringing object-glasses to the highest pitch of practical perfection.

From Mr. Carruthers we have received an account of the structure of Fossil Arborescent Lycopodiaceæ, valuable not only from the amount of accurate information it conveys, but as opening important views of the connections subsisting between different groups of plants.

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* This portion was prepared by the Secretaries.

Dr. Carpenter has placed on record a description and drawing of the ova of a fish, the parentage of which is unknown. He has also supplied interesting details of the structure of Fusulina, and the reparation of spines of Echinida.

Mr. E. Ray Lankester contributed a paper on Mr. Sheppard's Dichroic Fluid; and our late President during his last illness sent us another on Fluorescence *v.* Pseudo-dichroism, which has a melancholy interest as the closing work of his scientific career. Mr. Thomas Charters White also supplied some fresh details on Phycocyan at our last meeting, and which will be found in the February number of the Journal.

To Mr. S. J. McIntire we have been indebted for careful researches into the structure and appearances of scales of Thysanuræ; and Mr. Joseph Beck pointed out a valuable method of investigating the physical configuration of the markings on such bodies.

From Mr. W. Saville Kent we have received the earliest information and description of New Silicious Sponges, of great interest to the naturalist.

Mr. Slack brought before the Society the range of patterns to be found in artificial diatoms; the peculiar appearances of cracked films of colloid silica, and the action of colloid silica in modifying crystalline forms.

An interesting, and perhaps hitherto unrecognized creature was brought under the Society's notice by Mr. Barrett, and by him called a *Stentor*, though it is not probable naturalists will range it under that genus.

Mr. Bell contributed a series of researches on Fermentation and Ferments.

Mr. Hogg described and figured a Circaria parasitic on Lymnæus.

The Society is indebted to Mr. Lowne for details and drawings illustrating the anatomy of the Round Worm, and it may be safely affirmed that his elaborate and painstaking researches possess a very high degree of interest from the discoveries to which they have led, and which are regarded by comparative anatomists and physiologists as a valuable contribution to their sciences. Dr. Maddox has contributed further researches on Air Dust.

Turning from microscopical investigation to microscopical apparatus, the year has been remarkable for a new series of deep objectives by Messrs. Powell and Lealand with single front lenses, and capable of being adapted for dry or immersion use. It is sufficient to say that these powers exhibit a very marked advance upon the previous productions of these skilful artists. Immersion objectives have likewise been made by other distinguished English makers, and it cannot be said that the important principle involved in their construction is now neglected in this country.

Three new binoculars have been produced: one by Mr. Holmes, ingeniously formed of split lenses; one for high or low powers by Mr. Stephenson, on a plan quite different from that of Mr. Wenham; and one by Mr. Ahrens, for high powers, constructed upon a new principle with the aid of double-refracting spar.

For details of the optical construction of Mr. Stephenson's instrument, we must refer to his paper already published in the Journal. His plan gives, for the first time in a binocular, *erect* instead of inverted vision, which is often a matter of great convenience. Its action with high powers is really stereoscopic.

Dr. Carpenter contributed the comparative results, as regards steadiness at sea, of instruments of moderate weight and size on the Ross and Jackson models; and several minor matters have formed the subjects of communication.

The mere enumeration of the topics that have been brought before the Society during the past year will suffice to show the value of its operations; and in addition to the publication of its papers and 'Transactions' in the Monthly Journal, Dr. Lawson has ably carried out the wishes of the Council in connecting your Society with that publication, by bringing to a focus a great mass of microscopical research made by various English and foreign observers, and which were virtually inaccessible to ordinary students in their scattered state.

CONSPECTUS OF THE PRESENT STATE OF THE SOCIETY.

	Royal Patron.	Honorary and Foreign Fellows.	Associates.	Compounders.	£1. 1s. yearly.	£2. 2s. yearly.	Total.
Anniversary, 1870 ..	1	6	2	95	320	34	458
Since elected	..	..	..	+ 1	..	+ 20	+ 21
Since deceased	..	..	..	..	− 9	..	− 9
Since resigned	..	..	..	..	− 6	− 5	− 11
Anniversary, 1871 ..	1	6	2	96	305	49	459

BOOKS PURCHASED DURING THE YEAR.

'The Annals of Natural History,' 44 vols., or nearly all the first three series of the work, making it quite complete to the present time.

'Straus Durckheim Anatomie Comparée.'

'The Quarterly Journal of Microscopical Science,' vol. 18.

The numerous valuable donations have been periodically announced in the 'Monthly Microscopical Journal.'

II.—OBITUARY NOTICES.

Joseph Bancroft Reade was born on the 5th April, 1801, at Leeds, in Yorkshire, and received his elementary education at the grammar school of that town. He subsequently matriculated at Trinity College, Cambridge, whence he was elected to a scholarship at Caius College, graduating B.A. as Senior Optime in 1825, and M.A. in 1828. His first acquaintance with the microscope appears to have begun when he was fifteen years of age, his father at this time having presented him with one of Dolland's. The highest power of this microscope he preserved, and he wished it to be given to the Society after his decease, together with a microscope similar in make to the one referred to. It is evident that this presentation was an indication of the early bent of his mind. That he always showed great attachment to science, particularly to optics, is evidenced by the favourite studies of his life being astronomy and microscopy: chemistry and photography also came in for a share of his attention. The earliest published paper I can find, connected with microscopy, is one communicated to the 'Phil. Mag.,' in July, 1837, "On the Existence of Structure in the Ashes of Plants and their Analogy to the Osseous System of Animals."

The object of this paper was to prove, by microscopical examination of the ashes of plants, that combustion does not, as we have hitherto supposed, destroy brute matter merely, but that it leaves behind a purely vegetable product—a product far from being dissimilar in its nature to the bones of animals, and having its particles undoubtedly arranged by the "agency of a living principle."

To this paper some further observations were made in the November number of the same year, the great object of which was to show that there was no *accidental introduction* with respect to the elements of vegetable structure, as previously expressed by Braconnot; and with the view of showing that the organization of fossil plants is retained even after being submitted to the heat of a common fire, and that the white ashes of coal contained the usual forms of vegetable matter, *viz.* cellular structure, smooth and spiral fibre and annular ducts, illustrations of which accompanied the previous paper,—in this paper he further adverted to the structure observable in other specimens of coal, from which he inferred that the framework and basis of vegetable structure in the plants of coal is not only entirely independent of carbon, but that it has also resisted the bituminous decomposition which has converted all the carbonaceous material into a highly inflammable substance.

In the same journal (November, 1837) there is another paper, originally read at the British Association in 1837, "On the Chemical Composition of Vegetable Membrane and Fibre," in which he incidentally states that spiral vessels exist in the roots of dicotyledonous plants; and in the first volume of the 'Annals Nat. Hist.' he gives a detailed account of the statement above referred to, *viz.* the existence of spiral vessels in the roots of dicotyledonous plants, contrary to the hitherto received opinion of English botanists. They had been in the habit of considering spiral vessels as peculiar to the structure of monocotyledonous roots; and as proving a distinctive character between the root and stem of dicotyledons, he says, "So thoroughly has this opinion of their position gained credit that I have in no case been able to remove it but by giving ocular demonstration that it is in opposition to facts."

It was in 1837 also, whilst making some photographic experiments with the solar microscope, he discovered a mode hitherto unattained of separating the rays of heat from those of light, so as to enable pictures to be taken with cemented achromatic objectives with safety, and it was at this time he made the first micro-photographs; the best known of these are the head of a flea, and the section of the tooth of the Lamma, which were subsequently lithographed: a copy of that of the flea he has left to the Society, that of the Lamma forms one of the series selected by Owen for his Odontography; and it was whilst making these experiments that he discovered the value of gallic acid as a sensitizer, and hyposulphite of soda as a fixer on prepared paper, which stamps him as one of the foremost pioneers in the development of photography.

In the 'Annals Mag. of Nat. Hist.,' 1838, vol. ii., there is a paper by him "On some New Organic Remains in the Flint of Chalk," the object of which was to show more particularly the fossil contents of flint pebbles and flint nodules of the chalk, and to examine whether the flints of different strata had or had not a common origin. He found that Xanthidiæ were absent in the Brighton pebbles, whilst several species occurred in the flint of Kent and Surrey. Besides these singular remains which Ehrenberg had previously described, the author detected the scales of the ctenoid and cycloid orders of fishes which have not yet been found in earlier formations. For comparison, illustrations of scales of existing fish accompany the paper, and these were supplied to the author by Mr. Yarrell.

It was in this year (1838) that he was elected a Fellow of the Royal Society, and it was about this time that he drew the special attention of microscopists so strongly to the great value of black-ground illumination, that it has (though previously known) since gone by his name. In the first volume of the 'Transactions of the Microscopical Society' is a paper which he read before the Society, headed "The Process of charring Vegetable Tissue as applied to the Examination of the Stomata of Garden Rhubarb," in which he advocates the importance of the plan for determining the form and character of cells and cellular tissue, &c., and also as enabling the observer to say whether the stomata of plants are closed or open, a point at that time somewhat disputed.

In two communications to the Society in 1842, he drew attention to the great assistance which chemistry might derive from the use of the microscope, and goes on to state that a quantity of nitrogen, not exceeding the $\frac{1}{10000}$th part of a grain, if existing as a constituent of ammonia, may be detected with certainty by means of this instrument, and pointed out that ammonia was a product of respiration, a fact which did not obtain credence at the time, though justice was subsequently done to him on this point by Dr. Richardson, in his prize essay on the coagulation of the blood.

In the second volume of the 'Transactions' of the Society is another paper he read, "On Animals of the Chalk still found in the Living State in the Stomach of Oysters;" referring to the apparent identity between the fossil and living infusoria as of considerable interest to the geologist, in showing that there is a connecting link in the chain of organized beings of the same organic structure from the secondary formation to the tertiary, and to preclude the supposition that below the tertiary formation there are no recent species. Reasoning from the analogy of the recent oyster, the author examined the fossil oysters of the Kimmeridge clay, in which he met with similar forms as *coscinodiscus* and others corresponding to those existing in the recent oyster, so as to leave no doubt that they preserved the same beautiful mechanism, though we cannot now detect it, for making a current to set in the direction of the stomach, and he further infers that similar remains may be found in still older formations. The result shows how much we are indebted to the microscope for the confirmation of these views.

At the British Association, June, 1845, he read a paper "On Cilia and Ciliary Currents of the Oyster," the object of which was to show that the action of the ciliæ attached to the tentacles produces a strong current in the water, and thus that a number of minute living organisms (infusoria) was brought within their influence, and thereby affords a sufficient supply of food, thus compensating the prehensile organs possessed by higher creatures; and although it was well known that the orifice of the alimentary canal of the oyster was in the same manner fringed with ciliæ, it had not been suggested, says Mr. Reade, or proved by any naturalist, that the proper office of the ciliæ was to bring to these Acephalous molluscs that food which they had not the power otherwise to seize.

At the Exhibition of 1851 he exhibited an astronomical eyepiece, of his own invention, called a solid eye-piece, which was thought so highly of as to merit special mention.

In 1861 he read a paper before the Royal Microscopical Society, "On a New Hemispherical Condenser for the Microscope, and its use in illustrating an important principle in Microscopic Illumination," in which he describes a plan for getting from a single condenser two or more pencils of light at any angle to each other—a plan which commended itself much to those engaged in determining the markings of diatoms, "the principle sought to be carried out being to throw the axis of the pencil of illuminating rays in a direction at right angles to the line to be resolved," the great advantage of the plan being "simple, cheap, and easy of adjustment;" by means of it the dots on the *N. angulatum* are easily resolved with a half-inch object-glass. This condenser he subsequently improved by the addition first of one and then of another lens to the original Hemisphere, continuing his original plan for applying stops.

The subject of illumination in microscopic research was always a favourite study with him, and he has often been heard to say that the microscope was nothing without it, and it was from having this subject so constantly before him that he was led to the discovery of the value of a parallel beam of light in determining the true structure of the markings in diatoms and other minute objects. In June, 1869, he read a paper before the Society, descriptive of the prism which now goes by his name, the value of which cannot be over-estimated; for this inexpensive, simple appliance gives results which are not surpassed by any of the numerous contrivances of Wollaston, Brewster, Amici, Gillet, Kingsley, &c., or even his own favourite kettledrum, with flute-key adjustment, and it is interesting to note his own version of the step which led to its discovery and value. He says, "I very fortunately purchased of my friend, Mr. Powell, his little hemispherical condenser for showing the transverse lines of that prince of puzzles, the *Amphipleura pellucida*. In using this lens I saw on reflection that a small portion only was available. I therefore covered the whole lens with tinfoil, and then cut out opposite apertures on the plain and convex surface, so as to obtain, in point of fact, a small prism illuminator. This answered the purpose well, and it immediately occurred to me that the equilateral prism itself might be substituted for the prism slice of Powell's lens. I tried it, and saw at once the great advantages of the equilateral prism and its single parallel pencil of light. There are, indeed, many facts and secrets of structure which can be revealed by such a beam alone; light virtually parallel can lead to no confusion, but the crossing and recrossing of an infinite number of rays produce such a multiform shadow as to de-shadow or obliterate a true light and shade portraiture, which is the essence of every picture, and the very soul of every natural representation."

Dr. Donkin, who is now engaged in editing the 'Natural History of the British Diatomaceæ,' bears testimony to the great value of the prism in the examination of these interesting objects.

The prism is not only valuable for illumination of minute markings, but it can be used as a most effective reflecting polarizer, thus diminishing the cost of apparatus to the beginner.

His last contribution to science is an article in the 'Popular Science Review,' with illustrations, entitled "Microscopic Test-Objects seen under Parallel Light and Corrected Powers," in which he enters more into detail as to the value of the revelations made by the prism, and in this paper he strongly supports the views recently advanced by Dr. Royston-Pigott.

Such is a slight sketch of the published labours of the late President in connection with the microscope. As one of the founders of the Society, and one who for thirty years has always taken a warm interest in its success, he will be long remembered by all who knew him not only for his striking appearance, but for his kind and genial disposition, and his readiness to impart from the rich stores of his knowledge any information he possessed.

Arthur Raymond Betts, F.R.M.S., son of Dr. G. Harvey Betts, died of typhus fever caught in investigating the condition of Peyer's glands in the body of a patient he had attended suffering from that disease, and who died in Guy's Hospital. Mr. Betts had sedulously cultivated microscopic anatomy, and was highly complimented for his knowledge of it both at the College of Surgeons and at the London University. He made a fine collection of histological anatomy, and was engaged in forming a pathological one at the period of the fatal attack. From his skill as a working microscopist, and his scientific education, he gave promise of good work in enlarging the boundaries of knowledge, and while we lament his early loss we must honour the zeal that led to that exposure to contagion which is believed to have arrested his career.

John Bockett, F.R.M.S., was born in London, and educated for the medical profession, which he did not follow, but subsequently joined the staff at the Railway Clearing House, and became one of the principal clerks. He was an ardent microscopist, and took a great interest in all affairs connected with mechanical and optical science. A few weeks ago Mr. Bockett scratched his hand on an omnibus, his blood became poisoned, and he died rather suddenly on Saturday, the 28th of last month, aged 45.

The late R. J. Farrants, F.R.C.S. and F.R.M.S., an esteemed member of the medical profession, enjoyed a good and lucrative practice up to the period of his decease. The arduous nature of his duties, however, did not prevent him applying his mind to the acquirements of knowledge in a large and varied field of scientific inquiry; accordingly, among other sources or means of investigation connected with his profession, he was at a very early period led to recognize the importance and value of the microscope.

To those who had the advantage of his personal acquaintance it is needless to say that his indefatigable research, his patience and perseverance, aided by high mental qualifications, ensured a complete and satisfactory elucidation of the subjects which claimed his attention, and rendered his practical use of the microscope in the investigation of diseases as well as on other subjects most valuable. It would be travelling beyond the object of this short notice to allude to the contributions made by our deceased friend to medical literature or to the many services he has performed by his careful and exact analysis of matters submitted to him by medical friends. It is sufficient to say that with a retiring and modest disposition he was ever ready to give his valuable time and scientific knowledge and experience when and wherever it was desired or likely to be useful. Mr. Farrants was an old member of the Royal Microscopical Society; he was for many years elected to the Council, and in 1861–2 he was the President. At that period it was the good fortune of the Society to receive, through the medium of Mr. Farrants, that very valuable instrument for microscopical writing invented by his friend Mr. W. Peters. It is unnecessary to refer to the great pleasure every member of the Society has derived from the admirable manipulations of that instrument by the late Mr. Farrants; certainly so long as the instrument remains to adorn the Museum of the Society it will always be associated with the pleasant recollection of our departed friend, whose premature death at the age of 60 has deprived the Society of a valued member, and has left a void in a circle of friends which it will be difficult to supply.

"PETER JONES, F.R.M.S., late of Norton Folgate, was born in the year 1808. He was in his youth a student at the Birkbeck Mechanic's Institution, where he became remarkable for his attachment to the pursuit of natural history, especially botany; he also possessed a considerable knowledge of mineralogy and chemistry. Having passed through the usual period of studentship he became a lecturer on chemistry, in which he obtained considerable success. He afterwards established himself in Norton Folgate as a scientific and manufacturing chemist, and commenced business by giving a series of *conversaziones* at his house, which were rendered attractive by the exhibition of objects of natural history, and the employment of the microscope in their more minute investigation. He was elected a Fellow of the Royal Microscopical Society on the 12th of December, 1860. He died rather suddenly of angina pectoris on the 15th of March, 1870, in the 62nd year of his age."[*]

ELLIS GOODE LOBB, F.R.M.S., was born in 1807, at Canonbury Square, Islington, London. His father carried on an old-established business as an outfitter, at 148, Cheapside. Mr. Lobb was educated at St. Paul's School, and was intended for the law, but circumstances led him to follow his father's business, to which he succeeded about

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[*] 'Linnean Society's Journal.'

1844, and carried it on nearly to the time of his death. His inclination to science and literature was early manifested. He first took up astronomy, and, observing from the top of his house in Cheapside, made himself well acquainted with the constellations and planets. He afterwards taught himself Hebrew sufficiently to read and enjoy the Old Testament in the original. His occasional visits to the seaside led him to the use of the microscope, and subsequently his study of that instrument and its revelations withdrew his attention from all other recreative pursuits. He preferred Messrs. Powell and Lealand's microscopes to all others, and kept himself supplied with their latest improvements and finest objectives. His attention was devoted more particularly to what are called *test-objects*, and by great care in manipulation and illumination he was enabled to exhibit these, especially the closely-lined diatoms, to as great advantage as any contemporary microscopist. His knowledge of the groups of Desmidiæ and Diatomaceæ was precise and extensive, and he made himself fairly acquainted with other branches of natural history. His collection of objects was large, and the specimens were generally of great excellence. He studied the phenomena of polarized light, not only as applied to the microscope, but with the ordinary polariscope, and larger objects, including selenite designs, slices of crystals, &c., of which he had some beautiful examples. He sat for many years on the Council of the Royal Microscopical Society, and was one of the most constant attendants of both Council and ordinary meetings as long as his health permitted. He was warm and impulsive, but extremely good-natured, and he was always ready to assist others by showing them how to manipulate their instruments in the best manner by exhibiting choice objects and testing novel apparatus. He was much visited on this account, and highly respected for his kindness. He occasionally contributed short papers to the 'Transactions' of the Society, and frequently took part in its discussions.

For several years Mr. Lobb had suffered from rheumatism, acute and chronic, which made locomotion increasingly difficult, but he remained constant in attendance at the Society till about the middle of 1870. He then became almost helpless, and symptoms of paralysis appearing, he gave up business and removed to Selhurst, Surrey, where he died on September 9th, 1870, aged 63. He was interred at Abney Park Cemetery.

III.—*On some Recent Investigations into Minute Organisms.*

By Henry J. Slack, F.G.S., Sec. R.M.S.

In consequence of the lamented death of our esteemed President, the preparation of this portion of the Anniversary Address has been confided to me, and it seemed advisable to select for your consideration certain matters connected with minute organisms, which may not only prove of immediate interest, but also suggest many topics for further research. The microscopical work of each year assists in elevating the lower fungi, and the whole class of bodies to which such terms as ferments or "microzymes" are applied, into ever-increasing importance; and if the principal statements now asserted concerning them can be established, they must be deemed essential to the processes of life, to the production of various forms of disease, and to all the ordinary operations by which decay disintegrates dead masses, and prepares their constituents for fresh admission into the circle of organic being.

One distinguished inquirer, M. Béchamp, makes the startling announcement that blood corpuscles are aggregates of microzymes, or micro-ferments, and can give rise to chaplets of beads, bacteria, bacterides, and other forms. He further states that they behave like ferments, that they give birth to cells like leucocytes, and to smaller globules. These microzymes, he says, are capable of engendering cells in various media, and the blood corpuscle is the result of their work.* According to this view, respiration, which has long been regarded as a mode of nutrition, may be classed amongst the phenomena named fermentations. Such are M. Béchamp's conclusions.

In 1867 M. Béchamp, together with Messrs. Estor and Saintpierre, stated that the digestive action of saliva upon starch depended upon a ferment, and was not a simple chemical action. This ferment he describes as "secreted by the organisms of Leewenhoek," as they nourish themselves with the materials of the starch.†

In the germination of plants from seeds containing starch and other matters, a similar process is described by M. Le Maire, who states that vibriones and monads appear before the germination begins.‡ In 1870 M. L. Coutaret informed the French Academy that he had succeeded in showing that *maltine* or vegetable diastase obtained from barley possessed the same properties as salivary diastase; that it is a vegetable ptyaline identical with the animal one in physical, chemical, and physiological properties. In 1868 MM. Béchamp and Estor were led by a series of experiments to affirm that molecular granulations, or microzymes are universally contained in all cells, animal or vegetable, of which they regard them as normal and necessary constituents. These granulations, they say, can develop themselves into bacteria.

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* 'Comptes Rendus,' 7 Feb., 1870.

† C. R. for that year.

‡ 'Comptes Rendus,' 1863, Sec. Sem., p. 563.

The most startling of M. Béchamp's statements concerning these microzymes is, that they exist alive in chalk, or in other rocks. This announcement was made some time ago, but in the April of last year he detailed the results of a number of experiments to the French Academy, showing that the microzymes existed in a fresh-water tertiary limestone, as well as in chalk, in the same condition of "molecular granulations," and possessing the same powers of causing fermentation. He mentioned the *Calcaire d'Ormisson*, near Narbonne—lacustrine and middle tertiary; the *Calcaire de Borbentone*, near Beaucaire—middle tertiary and marine; the *Calcaire of Pignon*—middle tertiary and marine; the *Calcaire néocomien* de Lavallette, near Montpellier—very compact, and belonging to the lower chalk; and the *Calcaire oolithique* of the Meuse, as all capable of making cane-sugar and starch ferment. The tufaceous limestone of Castelnau, near Montpellier, contained the microzymes, but acted very slowly upon starch, so that at the end of two months, scarcely any products of fermentation could be traced—they confined their action to a very slow liquefaction of the starch. The same molecular ferments are found by him in fossil bones, but they are inactive at a temperature below from 35 to 40° C., so that they would not injure the fossil ivory which is exposed to severe cold. It would be interesting to know whether they exist in such ivory, and could be called into operation by suitable conditions of temperature and moisture.

M. Béchamp considers these objects as really living, and having been in the case of the limestones and chalk for unnumbered ages in a dormant state. Should further researches lead to the confirmation of this opinion, all tales of the germination of mummy wheat, &c., will be thrown into the shade.

No very precise data exist as to the length of time during which any kind of seed or germ can remain dormant. Amongst those cases which may be regarded as authentic, is one cited by Dr. Carpenter on the authority of Professor Lindley, in which three plants of raspberries were raised by the latter from seed taken from the stomach of a man, whose skeleton was found thirty feet below the surface of the earth, at the bottom of a barrow which was opened near Dorchester. He had been buried with some coins of the Emperor Hadrian, and it is probable, therefore, that the seeds were sixteen or seventeen hundred years old.* Many cases will occur to Fellows of the Society, in which seeds dug up at considerable depths have been stated to grow; and although the mind is not disposed without due examination to admit that germs, or organisms, have preserved their vitality during geological epochs, it would be difficult to assign any reason why mere lapse of time, however great, should necessarily destroy life capable of remaining dormant for a space like 1600 years.

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* Art. "Life," 'Cyclo. Anat. and Phys.,' vol. iii., p. 156.

With regard to the existence of organisms in minerals, reference may be made to the experiments of Mr. Staniland Wake, recorded in the March number (1870) of the 'Monthly Microscopical Journal.'

Fungi, and other minute organisms related to them, grow under many circumstances where their nutrition might have been declared impracticable, and preserve their vitality at temperatures which, without proof to the contrary, would be considered as necessarily destructive. Mr. Berkeley observes that solutions of arsenic, sulphate of iron, sulphate of copper, &c., though highly concentrated, do not prevent the growth of some fungi of a low order; and he cites the 'Nereis Borealis Americana,' to the effect that a few years ago a little mould was very troublesome to the department of Coast Survey at Washington, developing itself in copper solutions used for electrotyping, decomposing the salt, assimilating the acid, and precipitating the metal round its own threads. I have found a mould very fond of growing in saturated solutions of calcic and magnesic chlorides. The growth of an analogous mould in dialyzed solutions of silica in distilled water was brought under the notice of the Society some time back by Mr. Roberts and myself, and solutions of phosphate of soda in ordinary use are usually found by Professor Church in his laboratory in the Royal Agricultural College, Cirencester, to become green with a vegetable growth. In the solutions of citric and tartaric acids he finds colourless or grey plants appear. He states, in a note to me, "My experiments were chiefly made with these three liquids, and I obtained some very curious results by transferring the plant of one solution to another solution. On one occasion I removed all the phosphoric acid and potash from a solution of citric acid, to which I had added a little phosphate of potash, by introducing a fragment of a fungus from tartaric acid into it."

M. Béchamp mentions a very curious case in the following terms. He says: "I took very pure distilled water, and exposed it to the contact of air in a phial closed with paper. Colourless moulds appeared, formed of microzymes, very small bacteria, and an extremely fine mycelium. The apparatus was put on a stove and at the end of six months I obtained enough alcohol to give a large flame. At the same time a small quantity of volatile acid and of ammonia was formed." He asks: "Shall we say that distilled water, carbonic acid, and the elements of air have fermented? Evidently not; but we may say that the moulds grew and effected the synthesis of the materials composing their own substance, as all vegetables do, and that they then gave off the alcohol which they formed by the aid of this substance."

If other experimenters should be so fortunate as to grow the same fungus and confirm these results, new views will be opened concerning the nutrition of fungi, and perhaps some light may be thrown upon that of the yeast-plant, the precise action of which in the production of alcohol is still unknown. M. Béchamp speaks of his moulds as "dis-assimilating" the alcohol, as if it were an excretory product. It is well known that many varieties (or states) of fungi will produce the alcoholic fermentation, and many interesting facts of this description, together with other suggestive matter, will be found in the paper read by Mr. Bell before this Society and published in the July number of the Journal. Mr. Bell has hitherto obtained negative results in attempts to grow the common blue mould from the yeast-plant. Professor Hoffman said, in 1865, "When beer-yeast is cultivated with shelter against foreign germs, it gives rise to *Penicillium glaucum*, while the bakers' yeast, produced in brandy distilleries, and kept in an almost dry state, gives birth either to the same plant or to *Mucor racemosus* conjointly with it, or, more often, to the latter only. If we sow a number of the spores of these plants in a saccharine solution, as, for example, honey-water, we not only obtain a great quantity of pure carbonic acid until the sugar is decomposed, but also yeast, which yields the same products from which it was derived." *

M. Melsens made experiments last year on the vitality of beer-yeast. He found fermentation possible in the midst of melting ice, a temperature at which the yeast would not germinate. The life of the yeast-plant was not destroyed by the most intense cold that could be produced, about 100° C. below zero. In close vessels when the products of fermentation gave a pressure of about 25 atmospheres the process stopped, and the plant was killed. M. Boussingault, who was present when this communication was made to the French Academy, accepted the statement, on account of the known ability of M. Melsens, but he detailed experiments to show that other ferments had their activity destroyed by exposure to temperatures much less severe, or even by ordinary frost.

It is remarkable that M. Melsens obtained a similar result when experimenting with the supposed living particles of vaccine matter, on which its activity has been found to depend. He kept some vaccine matter for an hour and a half in sealed tubes exposed to the action of solid carbonic acid and ether, cooling it down to about 78° C., and the report of Dr. Jacobs, of the Veterinary College, Brussels, was as follows:—"Two tubes have been employed to vaccinate an infant seven months old. Five punctures gave five pustules. One tube, used on the same day to vaccinate an infant three months old, gave, with four punctures, three fine pustules."

The amount of heat necessary to destroy all germs is still in doubt. Some perish considerably below the temperature of boiling water, others are not injured at that heat. Some are not destroyed by rapidly passing through red-hot tubes, and possibly others may support for some time without injury temperatures of elevation corresponding to those of depression in the experiments with yeast and vaccine to which allusion has been made. Those who believe that our earth has passed through nebulous and molten stages, and also consider, with Professor Tyndall, that no new force or substance has been added, but that "life was present potentially in matter when in the nebulous form," might not deem any terrestrial heat capable of absolutely destroying this "potentiality," in whatever form it might ultimately reside. The microscopical student can only expect his special modes of investigation to throw an indirect light upon problems of this description, the solution of which may be altogether beyond the bounds of physical and chemical research; but it may be well to consider whether we may not encourage fallacies by too vague and general an employment of such terms as "life," "vital," "vitality," &c., and this is the more necessary when we watch or take part in the still unsettled controversy of the heterogenists. Some of the phenomena exhibited by what we call living organisms are obviously physical, others purely chemical, and we arrive at last at intelligence, thought, and manifestations of will, between which and any exhibition of physical, chemical or electrical force, we cannot trace the slightest connection of congruity or resemblance; nor does science offer, or even promise to offer, any explanation of the way in which they are connected with the action of nerve or brain. To refer all to that imaginary entity "vital force," would be an obvious blunder in logical method; and if we exclude mental operations, as being beyond the domain of mere "vital force," it will seem that we do not know anything that would justify ascribing the simplest life processes of such molecular organisms as M. Béchamp describes under the name of microzymes, and the complex relations and correlations of phenomena in the higher animals, to variations in the quantity of one vital principle. Dr. Lionel Beale, the most ardent supporter of the vital force doctrine still extant, says that "all truly *vital* phenomena must necessarily be altogether out of the range of mere physical investigation." Were this dictum worked into a definition of vital phenomena, it would exclude all that living beings do, or suffer, in accordance with physical or chemical laws; and new discoveries in synthetical chemistry would leave for vital force scarcely anything, perhaps nothing, but a method of directing and co-ordinating chemical and physical forces to vital ends. Science can make no use of, but must be injured by, terms that lack precision; and the objection to referring a particular action to "vital force" is, firstly, that no one can tell us exactly what is meant by the term, and secondly, that experience does not favour the reference of actions in living structures to a sort of force which no science can investigate, as each year adds to the number of operations performed by chemists, like Berthelot, with the ordinary apparatus of the laboratory, and resulting in the formation of substances which the vital-forcists had previously declared their special entity, only, could produce. Granting at once that there are mysteries in life, even of the humblest description, which defy all effort at scientific explanation, and frankly confessing where knowledge stops and absolute ignorance begins, we may still yield assent to the exclamations of M. Berthelot, in his 'Leçons sur les Methodes Générales de Synthèse en Chimie Organique.'[*] After stating that the progress of science is manifested in two lines, one that of philosophical ideas, and the other of practical application, he exclaims, "Shall I speak in the language of philosophy of those profound notions which chemistry affords concerning the constitution of matter, eternally durable, in the midst of perpetually changing appearances? What can be more striking than the conception of living beings, as formed by the assemblage of certain definite substances, comparable in their fundamental properties with mineral bodies formed of the same elements, obeying the same affinities, the same laws, chemical, physical, and mechanical: what more important than the reproduction of these substances, or first materials on which living organisms operate, by the sole play of mineral forces, and by the simple reaction of carbon on the elements of air and water? . . . . The general problems of the nutrition of living beings are chemical problems. It is the same with those of respiration. The study of these problems rests upon data supplied by organic chemistry. In the tissues of animals, as soon as the solids, the liquids, and the gases have been placed in reciprocal contact, under the influence of certain movements, resulting from the nervous system, and a special structure, which we know not how to imitate, affinities develop between these solids, liquids, and gases, which are purely chemical, and the combinations to which they give rise result exclusively from the laws of organic chemistry."

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* 'Comptes Rendus,' Prem. Sem., p. 632.

Reverting to the action of ferments, it may be remarked that fermentation was the subject of some lectures delivered by Dr. Williamson before the Society of Arts[†] last year. In reference to the microscopy of the organisms concerned in processes of fermentation, they can scarcely be considered as up to date, but it may be well to glance for a moment at the "conclusive" reasons alleged by the Professor for ranging the ferments amongst animals rather than plants. Dr. Williamson says: "These organisms assimilate, or, to use a homely phrase, they feed upon very complex substances; they give off during their vital functions less complex substances."

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[*] Pp. 8 and 9.

[†] 'Soc. Arts Journ.' 'Pharm. Journ.'

Another reason for considering them animals, he states to be, "that whereas plants require for their growth the light of the sun—in fact, their very growth is a process of absorption of heat by their leaves from the rays of the sun—and plants by so doing render heat latent, as we sometimes express it, that is, they cause an apparent disappearance of heat, and lower the temperature of surrounding space; animals, on the contrary, give off heat during the exercise of their vital functions, and do not need to be exposed to heat or to continuous light for their growth." This is surely an astonishing passage, and physiologists will feel compelled to demur to the assertion that heat is not necessary to the performance of the vital functions of animals, and that animals in general could live without light. If animals gave out all the heat they wanted, by their own processes, we might in this cool climate simplify clothing, abandon the domestic winter fire, and never have our benevolent sentiments shocked by tales of fellow-creatures frozen to death. It is certainly an error to say that "animals do not need to be exposed to heat," and also to represent plants as simply heat consumers, and not also heat producers. Mr. Grove, in his 'Correlation of Physical Forces,' says, "Heat is an immediate product of chemical affinity. I know of no exception to the general proposition, that all bodies in chemically combining produce heat." It is from this general law that plants do evolve heat in their processes of combination, and at times they evolve enough to produce a striking elevation of temperature, as is well known. It cannot be affirmed of all fungi, acting as ferments, that they feed upon complicated substances, though some do; and on the other hand, the food of other plants is not at all times, and always simple. For example, in the last edition of Dr. Carpenter's 'Principles of Human Physiology,' edited by Mr. Power, I find an allusion thus made to a paper by Risler, "On the Absorption of Humus by Plants," and on the functions of ordinary plants as compared with those of Fungi:—"The exhalation of carbonic acid is not peculiar to fungi or germinating embryos, for it takes place during the whole life of flowering plants, both by day and by night, in sunshine and in shade, and from their green as well as from their dark surfaces. And it is not improbable that, as in the case of fungi, its source lies partly in the organic matters absorbed; recent investigations having rendered it probable that plants really take up and assimilate soluble humus." Now soluble humus is a highly complex substance, probably consisting of various acids and ammonia, and requiring to part with some of its carbon to form the normal constituents of plants.

It is often a matter of interest to the microscopist to say whether organisms, occupying a sort of border-land, belong to the animal or the vegetable kingdom, and in many cases this cannot be done with precision or certainty. Some remarks of Boussingault will assist us to arrive at right conceptions of these matters. They will be found in a paper read before the French Academy, in 1864, on "Vegetation in Darkness."* He pointed out that the nutriment contained in seeds and in animal eggs was of the same description:—

Eggs.	Seeds.
"Albumen.	Albumen.
Fatty matters.	Fatty matters.
Milk sugar, glucose?	Starch, dextrine able to form glucose.
Sulphur, phosphorus, entering into organic compounds.	Sulphur, phosphorus, entering into organic compounds.
Phosphate of lime.	Phosphate of lime.
Water in great proportion.	Water in small proportion, cellulose."

The seed requires moisture and oxygen from the air, and its mode of germination is analogous to the incubation of the egg. Citing the 'Statique des Etres Organisés' by himself and M. Dumas, he observed: "We said (in 1841) at certain epochs, and in certain organs, the plant becomes an animal; that like an animal it becomes an apparatus for combustion; that it burns carbon and hydrogen; that it produces heat; that the sugar, or starch converted into sugar, furnishes the first materials by which this character is developed. The experiments I now bring before the Academy complete this statement, by showing that a plant developed in the dark, with stem, leaves and roots, behaves like an animal during the whole duration of its existence." M. Boussingault proceeded to show that while the animal, in addition to emitting heat and carbonic acid in respiration, modified by respiratory combustion a portion of the albumen it consumed into the nitrogenous crystalline compound urea, the plant growing in the dark transformed part of its albuminoid matter into a crystalline principle, asparagin, an amide like urea, and transformed into aspartate of ammonia with as much facility as urea becomes transformed into carbonate of ammonia. This asparagin is found in the juices of the cells, and thus differs from urea in not being an excretion. While the higher plants are not so sharply and completely separated from the animal kingdom as was formerly supposed, we must not be surprised if organisms belonging to, or related to fungi, and whose mode of nutrition bears strong resemblance to that of animals, should often be difficult to classify; and similar difficulties are experienced in dealing with some of the lower organisms living in ocean depths, and which seem to perform the functions of plants.

The connection of minute organisms of the nature of ferments with disease has occupied attention during the past as in preceding years. Dr. Lionel Beale, in a paper published in the last number of the Monthly Journal, protests against the opinion entertained by Mr. Simon and many others, that "each contagious disease is produced by a specific vegetable organism;" and he says it appears to him that the arguments break down as soon as they are analyzed, and the facts in their favour carefully investigated." Dr. Beale refers the mischief to minute living particles of "bioplasm," and he says, "these resemble one another in general appearance. Neither by its form, chemical composition, or other demonstrable properties, could the vaccine germ be distinguished from the small-pox germ, or the pus germ from either. All resemble the minute particles of the bioplasm of the blood from which they have probably been derived, but from which they differ so remarkably in power." The doctrine of absolute species propagated by special germs capable of no other development than one having correlation with a particular disease involves difficulties which require great consideration on the part of naturalists and physiologists, and is obviously connected with questions of the permanence of species, and the production of varieties. It may be doubted whether all living particles in plants, or which are plants, could be distinguished by any means with certainty from Dr. Beale's animal bioplasm particles; and we are not without recurring evidence that vegetable organisms may be causes of disease. Dr. Balestra* has lately adduced reasons for supposing that a microscopic plant producing an appearance like oil spots on the water of the Pontine Marshes, and which, on examination, is found to have a form like that of the *Cactus Peruviana*, is the source of ague poison. He describes its spores as $\frac{1}{1000}$th of a millimètre in diameter, and of a characteristic form. He found that small quantities of sulphate of quinine and arsenious acid or sulphite of soda altered them in a striking way and killed them. He traced them in the marsh air, and believed that an ague with which he was twice attacked was occasioned by inhaling them—the fit coming on after having involuntarily smelt water in fermentation and covered with the plants and their spores.

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* 'C. R.,' 1864, Prem. Sem., 917.

The valuable researches of M. Pasteur and others into the cause of silkworm diseases are too well known to need extensive reference, and by the methods he proposed of obtaining eggs from healthy moths kept in seclusion, an enormous gain has been secured to the cultivators. M. Pasteur distinguishes three principal silkworm diseases, *muscardine* produced by spores of *Botrytis Bassiana*, and *pébrine* produced by "corpuscles," found in all stages from the egg to the moth, in all its tissues and liquids, in the silk material, and in the dejections. These corpuscles, he tells us, multiply by germs, but those which are found in the dust and rubbish of silk farms, he says, are dried up and deprived of life. Those which find their way into eggs are active for mischief. They may be introduced by birth and descent, by inoculation, or by food. Another disorder called *la flâcherie* is produced by a ferment which develops chaplets of beads. These bodies have their origin in a fermentation of the mulberry-leaves. The investigations of M. Pasteur establish the fact that bodies belonging to the class of ferments can be causes of disease, and they afford a striking instance of the value of the microscope in detecting such causes, and checking their action. The chief silkworm diseases arise from objects which the microscopist furnished with the data supplied by Cornalia, Quatrefages, and Pasteur can recognize with certainty, but the same cannot be said of other bodies known or supposed to have morbific properties, some of which are alike in appearance, while others, more susceptible of discrimination, bewilder an investigator when he tries to trace certain consequences back to them as an efficient cause.

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* 'Comptes Rendus,' 18 July, 1870, p. 235.

Dr. Burdon Sanderson considers it probable that "every kind of contagion consists of particles;" but then come the questions of What are these particles? What is their origin? and How can we discriminate one from another. Referring to Hallier's researches and discovery of colonies of micrococci in every contagious liquid, he observes: "Assuming this observation to be correct, and that, as Hallier believes, the microzymes are identical with the contagious particles, it is of no value as a means of distinguishing the various contagia from each other, unless the micrococci are capable of differentiation;" which last words seemed to be used inadvertently for susceptible *of discrimination.* "Hallier," he continues, "admits at once that no specific distinctions can be founded on their forms or appearances, as actually observed in contagious liquids. He seeks for the required characteristics in their development, and maintains that, although the microzymes of two different classes are exactly the same, the higher forms to which they severally unfold are specifically distinct." He goes farther than this: from the higher forms obtained by cultivation he claims to be able to reproduce contagious microzymes. Dr. Sanderson does not consider that Hallier has proved his case, and this seems to be the general opinion in this country; but what is here said may induce Fellows of this Society to make experiments with the lower forms of life, and work out disputed or doubtful points of fungoid development. Among the fungi to which Hallier imputes disease is the *Pleospora herbarum*, the form that is parasitical on darnel being in his opinion the cause of sheep-pox—a thing which Dr. Sanderson observes has not been tested by actual experiment.

Whether we call the organic particles in question "bioplasts," or "microzymes," whether we suppose them animal or vegetable, and whatever theory we form as to their origin, there are three conspicuous modes by which they may become disseminated, *viz.* by water, or by air, or in food either liquid or solid. The Yearly Report on Public Health, by Mr. Simon, published last year, contains a paper by Dr. Thorne, "On the Effects produced on the

Human Subject by Consumption of Milk from Cows having Foot-and-Mouth Disease," and his conclusions are that "a disease appears sometimes to have been produced in the human subject when the milk of cows suffering from the disease has been freely used without being boiled;" but he adds, "in a very large number of cases the milk of cows undoubtedly affected has been used without producing any noticeable marked effects." This seems to be analogous to the well-known fact that persons or animals exposed to contagion do not always catch the diseases which those contagions are supposed competent to produce; and this suggests the important inquiry as to what conditions the contagions require for their mischievous activity, and also whether microzymes, which under some circumstances produce disease by their processes of growth or development, can develop under other conditions without inducing such results. In the case of cow's milk infected with germs of foot-and-mouth disease, Londoners have been supposed to have derived advantage from the well-known practice of the trade to distribute their article in the mild form of milk and water; but the addition of the aqueous fluid is not always contributive to our safety, and Dr. Ballard has recently published a pamphlet detailing a series of cautious researches, which render it probable that a fatal outbreak of typhoid fever in Islington may have originated through the milk of a particular establishment being mixed with water from an underground tank, which by the work of rats had been made to communicate with some old drains. Unfortunately, no microscopic examination of the water from this tank was made at the time of the fever, as its existence was not then known, and at a later date Dr. Bernays stated that its analysis coincided with that of the New River Company supply.

That air conveys with its currents to various places germs, spores, and particles of different substances, has been long known; but the general public has been led to take much interest in the matter, and perhaps to entertain exaggerated views of danger, through Professor Tyndall's lectures and exhibition by the electric light of the particles which constitute the motes in the sunbeams, and which have been gazed at for generations. Some very interesting researches on the particles floating in air will be found in Pouchet's 'Heterogénie,' published in 1859, under the head of "Micrographie Atmospherique," in which he shows that the greater portion of these particles consists of small fragments of various materials exposed to friction and disintegration, and by no means comprises the quantity of eggs, spores, &c., that some have imagined. In other investigations it was found that the air of rural districts contained, as might be expected, fewer particles than that of towns, and mountain regions were almost free from them.

Dr. Maddox brought this question before us in a paper which was published in the Journal for June, 1870, with drawings of an apparatus employed to collect the particles, and of the objects he found, and he has furnished us with further particulars.* Dr. Maddox lives near Southampton, and the result of examining the collections of atmospheric particles for forty days, showed that "the air cannot be considered as *loaded* with microscopic germs; the largest number visible and counted as such on one cover being twenty-one (not including bacteroid bodies)." In the August (1870) number of the Journal, Mr. Metcalfe Johnson, of Lancaster, described an "Air-Sieve," and stated that he found varying quantities of *Monas lens*, and of organic matter. The presence of monads he found best ascertained by examining the surface of the distilled water, which in his apparatus trickles slowly over a glass plate in contact with the air. In the same number, Dr. Sigerson describes results of his examinations, and gives a variety of instructing particulars coinciding in the main with the researches of Pouchet. In iron-works he found curious little bodies, minute bombs or balloons, from 1—500″ to 1—5000″ in diameter. One of his conclusions is, that "the theory of the panspermists seems unfounded on fact—that there are no hosts of germs *always* floating about in the atmosphere, invisible and maleficent as genii in Eastern stories." Several times he "came upon atmospheres where a sunbeam could not be seen for want of motes." He considers contagions to consist of albumenized matter in a state of motor change, and he speaks of granules, resembling exudation granules, found in a fever atmosphere.

In the 'Proceedings of the Bristol Naturalists' Society' for 1869, is a paper by Mr. Stoddart, on the contents of rain water collected in Bristol, showing the matter brought down from the atmosphere by descending showers. One specimen collected in North Street contained 10·31 grains of solid matter per gallon, including sulphates and chlorides of ammonia and soda, nitrate of ammonia, pure sulphuric acid and carbonaceous matter and greasy organic matter, the last to the extent of 2·63. A gallon collected in King's Square yielded 6·76 grains of solid matter, the "greasy organic matter" amounting to 1·43. It is to be regretted that Mr. Stoddart did not add the results of a microscopic examination.

From the consideration of the matters floating in air, we pass naturally to the question of Spontaneous Generation, or as it is better to call it, *Heterogenesis*, which still engages the attention of many microscopists, and cannot be affirmed to be decided. Professor Huxley made this subject the topic of the Address he delivered to the British Association last September, in Liverpool; and although it would certainly be possible to place the evidence adduced by the "Abiogenists" in a stronger position than he has done, the majority of those who have paid attention to the controversy or engaged in experiments, will coincide with his statement that "with organic chemistry, molecular physics, and physiology yet in their infancy, and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call 'vital' may not some day be artificially brought together. All that I feel justified in affirming is, that I see no reason for believing that the fact has been performed yet."

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* See Journal for February.

Dr. Bastian's experiments and statements have been the subject of brisk controversy, and he cannot escape the charge of haste, and in some cases want of sufficient care. Cautious experiments made by others have shown that glass vessels are acted upon by such solutions as he employed; and Dr. Frankland describes in 'Nature' (January 19th) glass splinters and spheroidal bodies, which he says were evidently rounded particles of glass, obtained by exposing carbonate of ammonia 15 grains, phosphate of soda 5 grains, distilled water 1 ounce, to a high temperature in tubes of hard Bohemian glass. Dr. Frankland observes that the movement of certain particles was evidently Brownian; but without doubting that his particles possessed no life, we may ask is that sort of motion any proof of it? Would not living bodies, if small enough in proportion to their specific gravity, be affected with this motion just as inorganic bodies of the same weight, size, and form would be?

Theoretical conclusions such as those resulting from a consideration of the Pangenesis hypothesis of Darwin, together with inferences from a host of facts, lead us to believe that vital properties may exist in particles too small for microscopic investigation, or even visibility. Dr. Pigott estimates the spherical error of what we have considered fine objectives as the 1—50,000″; and we have glimpses of bodies less than that size which are alive, and the structures of which must be far too minute for examination by any instruments we can at present hope to possess. We are liable not only to be baffled by minuteness of size, but also by close approximations to homogeneity of structure, colour, and refractive power. We want to see the ultimate molecular structure of bodies on which dialytic and perhaps chemical actions depend, and we have no means of doing it. The controversy between heterogenists and other thinkers may thus elude all direct research, but it would, on all grounds, be a mistake to discourage investigations, because they are carried on in the hope of establishing conclusions that militate against existing ideas, or theories that have been long accepted. We must in our special pursuits avoid the method of reasoning resorted to in old times by supporters of incorrect theories of astronomy, and which is still occasionally used by men of some pretensions to scientific knowledge. To put such reasoning in a logical form, it would stand thus:—The Creator assigned the best orbits to planets; circular orbits are the best, therefore the orbits of planets are circular. We must not assume that we know all about the origin of life, and say, "the best principle is that of *omne vivum ex ovo* or *ex vivo*, therefore such is the origin of all living things, subsequent to a primal act of creation." We may not venture to say how, but we may be sure that in the future, discoveries will be made that will deal with many opinions we now hold, as past discoveries dealt with the errors that preceded them. Science comes with startling surprises to the most gifted minds, and let us hope it may be ours to hear without alarm what Tennyson calls that

> "Bridal dawn of thunder-peals, which comes
> Whenever thought hath wedded fact."

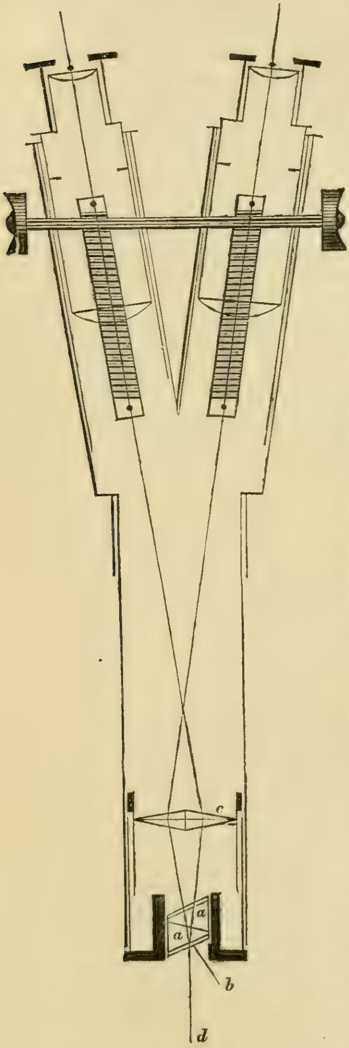
IV.—*On a New Form of Binocular Eye-piece and Binocular Microscope for High Powers.* By C. D. Ahrens.

(*Taken as read before the* Royal Microscopical Society, *Feb.* 8.)

It is now about four years ago since I made a binocular microscope, the prism of which was made of Iceland spar. It was not made public at the time through any of the journals, as I could not get it to define so perfectly as I wished. There were, however, several gentlemen to whom I showed it at the time, and explained its construction; among these were Dr. Millar, Mr. Wenham, and several others. Mr. Wenham then referred to his having tried to make the same some years previously, and said that I had been more successful than he was. By means of much labour and perseverance I have, since the time referred to, brought the idea to a successful issue, one which at starting I had scarcely contemplated.

Through having been called upon to make many hundreds of prisms for the beautiful arrangement by Mr. Wenham, the subject of binocular vision is one in which I feel an especial degree of interest, and in consequence I have been led to try if there were not some other combination that could be made, by means of which we might use the high powers as well as the lower ones, and still have a stereoscopic effect, both fields being equally and evenly illuminated without any extra or special illumination. I believe that I have now succeeded in doing so.

The means of effecting this is, as I said, obtained through the agency of Iceland spar. I make a double-image prism out of spar alone, for I get just double the amount of separation of the rays that I should from one of glass and spar. I cut the spar in a way that would be somewhat difficult for me to explain satisfactorily. I grind one piece to an angle of about 40°, and then cut it in two, and reverse them, placing the thickest parts opposite each other, and cementing them together with balsam. At the same time I cement in a similar manner a piece of thin parallel glass on each end, to preserve the surface; and I find that by this addition I also get better definition. In each of these pieces of spar there are two images, the ordinary and the extraordinary rays; and when these two pieces are balsamed together we can only see two, that is the extraordinary rays that I make use of, the two ordinary ones being shut off; and I get two perfect uniform images of the same separation out of one piece of spar. This prism divides the rays about $2\frac{1}{8}$ inches in 8 inches, which I find very convenient for the microscope, allowing rack-work for the different widths of sights. Of course there is a great deal of colour in each of these rays, but I do away with this by placing a double-wedge flint glass prism of an angle of 25° (as shown in the drawing) over the double-image prisms, and as close as I can get them just to allow the fields to be illuminated all over; and these prisms cause the rays to cross; and being very close to the double-image ones we do not get all the rays of light, only sufficient to be illuminated, which gives it a very good stereoscopic effect; and these flint prisms bring the centre rays about the same distance apart as they were before they crossed.



In this arrangement you require a special pair of bodies, both being at an angle. This is the best plan, because you do not lose so much light, being closer down than the binocular eye-piece I have made, for I was asked by several gentlemen if I could not make a binocular eye-piece for high powers, as they did not wish to have any more alterations made on their present form of microscopes. This I have succeeded in doing. I make the prisms go down the upright tube about 5 inches, and the rays cross in this tube as represented in the drawing, and about 4½ inches above the upright tube. This is a severe test for it, the rays travelling so far; loss of light we can make up for by having a stronger light or a larger mirror.

This arrangement may not be quite so good as Mr. Wenham's for *low* powers, but for *high* powers I cannot conceive of anything finer.

This system also gives one of the most perfect micro-polariscopes that we can have, for the double-image prism takes the place of the Analyser; and the prisms being worked so very true compared with what the Nicol's prisms in general are, we get the full field, but we can use only one body at the time unless there is a Nicol's prism in

EXPLANATION OF THE FIGURE.

a, *a*, Double-image prism of calc-spar, lined with glass *b*. *c*, Double flint prism for achromatism of the light. *d*, The ray of light from the object-glass. Rack-work and milled head are shown.

one of the tubes. This is because of the way in which the double-image prisms are ground; that is, equal to one Nicol's prism turned round 90°; hence you cannot get the same colour in each field unless, as I say, a Nicol's prism is placed in one of the tubes or on the top of the eye-piece.

I think that this binocular eye-piece might be used with advantage in the telescope, for by its means we could tell whether the object examined shone by its own light or by reflected light.

V.—*On Crystalline Forms modified by Colloid Silica.*

By Henry James Slack, F.G.S., Sec. R.M.S.

(*Taken as read before the* Royal Microscopical Society, *Feb.* 8.)

Plates LXXVII. and LXXVIII.

The remarks in this paper must be regarded as in continuation of those previously communicated to the Society. The nature of the action of colloid silica in modifying the patterns obtained in preparing various crystals for observation with the microscope and polarizing apparatus, has already been explained, and we may at once proceed to the consideration of the figures in the accompanying Plates.

Fig. 1 represents a portion of a pattern obtained by heating and quickly drying a drop of a strong solution of cadmic chloride prepared with distilled water containing colloid silica. The whole pattern is approximately circular — what is shown is included between two radii and a curved line near the outer circumference, about 1—10″. In this slide crystallization has suffered extreme interruption, and the result is a deposition of the salt in a form rudely, but strikingly resembling the patterns of sections of certain echinus spines. In the centre are some clear cellular-looking spaces, and on all sides proceed radial lines, with concentric lines at intervals. Fig. 10 shows the same salt in a crystalline flower pattern.

Fig. 2. Hippuric acid prepared with the silica. Here we see the action of radial and tangential forces. The spirals, in bands from 1—170″ to 1—200″, appear to be thickened and slightly-raised portions of the general structure, which is a modification of the well-known grouping of radiating needles into circular patterns.

The strong lines show silica cracks.

Fig. 3, hippuric acid and silica, exhibits graceful floral curves, in which the angular character of simple and pure crystallization is modified into organic outlines like those of exquisitely-shaped flowers and foliage. High-power views ($\frac{1}{8}$th and A or B eye-piece) exhibit multitudes of minute markings, cracks, and modifications of level. Size from about 1—30″ to 1—100″.

Fig. 4 is taken from a slide of hippuric acid and silica. This slide contains circular groups of various sizes with curiously-frilled borders springing from the points of elongated needles. Most of the patterns run into each other, and they all show Maltese crosses of colour with the polariscope. The artist has shown small rosette patterns from this slide. Some rosettes as small as 1—400″, others 1—100″.

Fig. 5. Potassic chlorate and silica. Some of the various patterns obtained with this salt. The architectural, pedestal-looking group exhibits curves as well as angles. Many flat crystals show silica cracks, which look as if engraved or incised: they do not go through the substance. Pedestal group about 1—40″.

Fig. 6. Cupric sulphate and silica. This contains figures selected from a variety of patterns. The sketch exhibits a curved floral pattern of considerable complexity and of great beauty with the polariscope, and also small rosettes. The latter, with high-powers, is seen full of minute workings, approximately like diatom beadings. Rosettes from 1—50″ to 1—20″.

Fig. 7. Cupric sulphate and silica. A nautiloid pattern, composed of curved lines, the chief arrangement of which is from right to left, as seen in an inverting instrument, with parts running the opposite way. The imbricated scale or feather-like aspect results from silica cracks and modifications of level and thickness. In this slide also is shown what in former notes was designated the "pavement pattern." Nautiloid pattern rather less than 1—40″ long diameter.

Fig. 8. Cupric sulphate and silica. Complicated spirals, with minute bead-like divisions. Many of the spirals in this slide run into each other and are confused. The circles containing the spirals up to about 1—34″.

Fig. 9. Salicine and silica. Small flower patterns. Many of them, with $\frac{1}{8}$th and A or B eye-piece, exhibit radial lines of minute beads and granules, arranged to produce effects most like actinocyclus. Small ones 1—400″.

Fig. 10. Cadmic chloride and silica. See remark at the end of description of Fig. 1. Size from rather more than 1—100″ to very minute.

Fig. 11. Tartaric acid and silica. This combination gives a mixture of irregular pavement pattern, with more or less floral patterns.

Fig. 12. Salicine and silica. In this slide the tangential forces have developed the radial patterns seen in common slides, with flowering curves like fine foliage. The variations in thickness, &c., give rise to a greater range of tint and more colour harmony than in ordinary slides. Floral pattern about 1—8″.

Fig. 13. The patterns in this slide are extremely complicated and elegant. Very delicate fan-shaped radial groups give the appearance of a delicate tissue. They spring from needles, and are exquisitely fringed. Some complete circles about 1—50″.

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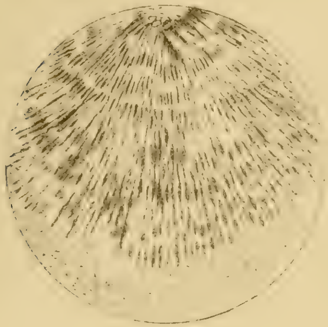

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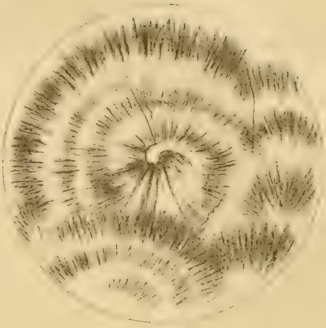

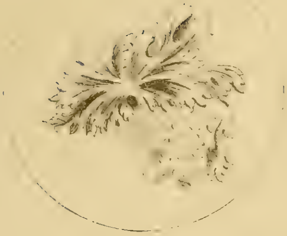

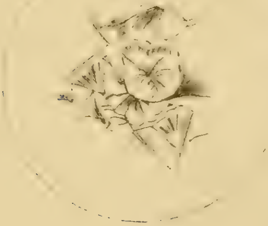

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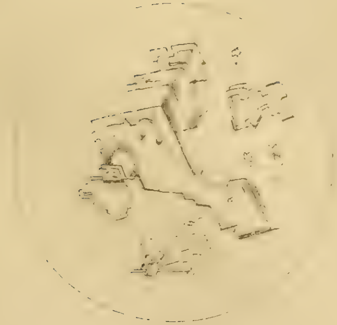

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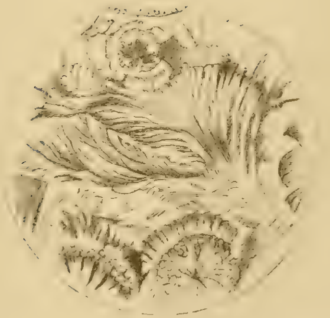

Copper. 2.

Tuffen West sc.

Crystals Modified by Colloid Silica.

VI.—*Object-glasses and their Definition.*

By F. H. Wenham, Vice-President R.M.S.

I may offer some excuse or apology for the controversy under the above head with Dr. Pigott. Why he should state that he has incurred my displeasure on the items recapitulated in the last Journal I am at a loss to imagine, as I have not been out of temper, or had any feeling of the kind. At the outset he made some statements concerning object-glasses, which appeared to me questionable, and fairly threw down the gauntlet by saying that "the battle of the glasses will have to be fought." All these questions must be resolved in a practical sense at last. It could not be expected that any of the professional makers would express opinions, or divulge their practice; and seeing grounds for objection, I accepted the challenge, and during the controversy, or "battle," in the usual course I bring forward any facts that will tend to prove the error. With every desire to arrive at the truth this is oftentimes a source of irritation. Events have shown that the first points need not have been dwelt upon. I might well have left the beaded appearance of the *Podura* to the decision of others. I have lately looked over a great number of slides, and now have particular specimens which easily develop beads with a ½ inch. Traced to its source with the highest powers, this is evidently caused by intercostal corrugations of the membrane. I therefore endorse the opinion of the most diligent and accurate observer of this class of objects, that the beads are "ghost beads," and the ! ! ! spines are not "*spurious*," as Dr. Pigott states. We are all working with the same view of obtaining results, and must admire the industry with which Dr. Pigott has been elucidating the primary laws of refraction and reflexion, by his papers in the Journal and 'Transactions' of the Society during the past year; and though the caustic curves, and figures of aberration from a variety of concave and convex reflecting surfaces, are to be found in Potter's and other treatises on Optics, still the rehearsals may be welcome to those who do not habitually refer to these works.

With respect to the immersion lens (the true advantage of which I have never disputed),* I must state that Figs. 2 and 3, page 23, Jan. 1871, are not mere imaginative diagrams arranged to meet a far-stretched theory, but transcribe a positive fact. All the distances there shown, both for thickness, radius, diameter, and focal distance, were taken from actual and repeatedly-verified measurements, when adjusted for the *same* object and cover. The aperture remains the same in each, and of course if Canada balsam is run under the cover to mount the object, the aperture will be equally reduced thereon in both cases. This has been shown years ago. That the mere fusing by a refractive medium of the thinner or immersion lens and cover into one, whose combined thickness barely exceeds that of the dry lens and cover together, will be the means of collecting a greater angle of rays from the object, is a fallacy that must be obvious, even though my friend in Egypt, the great Ptolemy, is now brought up as evidence against me; and whether the object is mounted in balsam or not—I challenge Dr. Pigott, or anyone, to get, through the object-glass with the immersion front, a greater angle, or any portion of the extraneous rays that would in the other case be totally reflected, as no object-glass can collect image-forming rays beyond this limit.

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* It might be inferred from the tone of Dr. Pigott's communications, that he defends the immersion principle against a number of opponents.—Where are they?

I now leave it to impartial observers to consider my objections. I have nothing more to say concerning the immersion lens, and in this particular the controversy with Dr. Pigott must end—if only for the tone last assumed—that of mutilated quotations, imputed "personalities"? want of "refinement," &c. I might rebut these insinuations by referring to the November number of this Journal for some expressions, first emanating from himself.

I trust that I have not hitherto tried the patience of the kind readers of this Journal. It is not my forte to write on any but practical subjects, and in my last I gave the marginal rays traced through an entire object-glass. Little or nothing of this kind has been previously published. I expect shortly to be able to carry out some experiments that I have in view, and to give another illustration, I hope to better purpose.

VII.—*Nobert's Nineteenth Band and its Observers.*

By Charles Stodder.

J. J. Woodward, Assistant-Surgeon and Brevet Lieut.-Colonel U. S. Army, having, in a letter published in this Journal in the August issue, affirmed his disbelief in the fact claimed by myself and Mr. R. C. Greenleaf, of the resolution of the lines of the 19th band of the Nobert test-plate, and his disbelief that I ever saw the lines with the instruments named, I wish to make a rejoinder which shall close the discussion on my part in regard to that matter.

Dr. Woodward writes, that "if Messrs. Stodder and Greenleaf's original claims were really well founded, they certainly ante-dated mine." I have already stated (this Journal, May, 1870) that our original claim rests on our belief, no one else having seen the resolution with the instrument then used. But in my letter I then claimed, and not for the first time, to have ante-dated Dr. W.'s claims nearly a year, independent of the original claim of 1867; and I now repeat my claim, that I and others repeatedly saw the true lines of the 19th band with *several of Tolles' objectives* (no one of them being of so high a power as a $\frac{1}{16}$th) previous to the time that they were seen by him, after all due consideration of his reasons for disbelieving me.

Dr. Woodward's reason for refusing belief in my claim is this: "In my judgment it cannot be said that one of the bands is resolved, unless the lines are shown in such a manner that they can be correctly counted from one edge of the band to the other." This sentence contains the whole gist of the matter. (It will be interesting hereafter to examine the evidence offered that Dr. W. has himself performed that feat.) Dr. Woodward here assumes that "his judgment" is conclusive and sufficient, and that that settles the matter; that all who dissent from that "judgment" must be in error. It is certainly a short method of settling disputed questions, but a method that is not generally accepted. I acknowledge that Dr. Woodward has obtained a high reputation as a microscopist, that his instruments, apparatus, and facilities for observing are of a high order, having almost at his command the purse of the United States; that the photographs executed by him, or under his direction, are, many of them, of surpassing excellence; yet I cannot admit that his "judgment" is sufficient authority, or competent to establish the law for the microscopical world against the "judgment" of other experts.

Mr. W. S. Sulivant says,* "a portion only of the width [of a band] can be brought into exact focus at once; if that portion, however, is measured, and its lines resolved under suitable amplification, the data are obtained for the solution of the problem." Mr. Sulivant's authority on this question is second to none: the method he requires has been repeatedly applied to the resolution with Tolles' objectives, but I did not deem it of importance enough to mention. I have conferred with numerous experts on this matter. Dr. Woodward's law is generally rejected. That ought to suffice; but as Dr. Woodward has produced one "witness" to support his "judgment," before I close I propose to introduce some on my side.

Dr. Woodward makes some trite observations on the fact "that objectives of inadequate defining power can easily be made to show" spurious lines—a fact to be noticed by all novices, but which it was hardly worth while to repeat to me, as Mr. Greenleaf and myself were perfectly familiar with the fact, having worked on the Nobert plates for years, and resolved the equivalent of the 15th band with a Tolles' $\frac{1}{5}$th in the year (I think) 1865. Dr. W. might have added that not only "objectives of inadequate defining power," but the very best will show spurious lines if they are not properly manipulated. He says, "Now not only did Mr. Stodder in his original paper admit that he had not counted the lines, but he fell into a grave error on the subject of counting fine lines, which he expresses in the following words:—'In counting lines of such exquisite fineness, either the micrometer or the stage must be moved,'" &c. Had it occurred to me when writing that anyone could have possibly supposed that I referred to anything except such "exquisitely fine lines" as I *saw them* with only 550 diameters, I might have been a little more explicit; but as I applied the words, they were strictly correct, and I do not retract them. I "fell into" no "grave error"—no error whatever; and of that Dr. Woodward's account of his own experience is sufficient proof. He says, "On attempting to count the lines, however, with a good cobweb micrometer made by Stackpole, of New York, *I found myself unable to get beyond the 9th or 10th band, on account of the tremor communicated to the instrument when the micrometer screw was turned!!*"* This is a full confirmation of my remarks by Dr. Woodward himself. I can add that Dr. Barnard, of New York, encountered the same difficulty. I will also say, at that time neither Mr. Greenleaf nor myself had *any* apparatus to assist in counting, and that then I had never even seen a cobweb micrometer.

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* 'American Journ. of Science,' vol. xlvi., p. 350, and also in letter to me.

Dr. Woodward has been able to find one indorser of his "judgment," Dr. H. Hagen, of Cambridge, an eminent *entomologist.* After an examination of Dr. Hagen, and finding their "judgments" to coincide, Dr. Woodward pronounces him to be "a competent witness." Dr. Hagen has published in Max Schultze's 'Archiv' (second No., 1870), a paper "On American Microscopes." In that he relates how Mr. Tolles undertook to show him the 19th band with an immersion $\frac{1}{10}$th. Dr. Woodward renders Dr. Hagen's account in this way:—"He showed him lines, indeed, but he was unable to count more than forty of them. Mr. Tolles himself counted between forty and fifty of them. These counts show that the lines in question were spurious." Fortunately I have a copy of the 'Archiv.' The correct reading of the passage is (as translated by a German), "An objective of $\frac{1}{10}$th in focus showed, while band 19 was in the centre of the field, the 18th, 17th, and half of the 16th bands. The lines *in all were well defined*, but not so that I could have counted them all. I could count about forty of the 19th, the REST blurred." This is very different from Dr. W.'s reading, which is, that the forty counted were the whole width of the band, and consequently spurious. I am authorized to say that Dr. Hagen has stated in conversation that he saw the *true* lines. In fact, Dr. H. saw the 19th band resolved, though he does not admit it, because he did not count all the lines. As Dr. Hagen has been replied to,* the reply to him will answer also for Dr. W. The position assumed by both gentlemen is absurd; for "suppose that Nobert had ruled in the 19th band only twenty-eight lines instead of fifty-seven, would Dr. H. say that they were not resolved because there were no more? Or if Nobert had covered a whole inch with the 112,000 and some odd lines, would anyone claim that all must be seen at once? If either of these suggestions is answered in the negative, then Dr. Hagen has himself seen the 19th band resolved with a Tolles' objective."

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* 'Quart. Journ. Mic. Science,' Oct., 1868, p. 229.

The evidence of the resolution of the lines is in the progressively increasing fineness of each band, as each is successively resolved; and in the experience of the observer, in distinguishing the difference between diffraction, spectral, or spurious, and the true lines.

I will now introduce my witnesses, who are tolerably well known among microscopists, so that I hope Dr. Woodward will admit their "competency." I am forced as it were to this course by Dr. Woodward's persistence in advertising that he does "not believe" me.

First, a letter from Mr. Edwin Bicknell, of the Zoological Museum, Cambridge.

Cambridge, *Sept.* 8, 1870.

Dear Sir,—I desire to state that in August, 1868, I called at Mr. Tolles' room in Hanover Street, and while there had an opportunity of viewing the test-plate of Nobert. I *distinctly* saw the 19th band (if I can trust my eyes) well resolved, the lines sharp and distinct; with no special lines, and each line of the band much narrower than each space between the lines. While I regard counting a positive proof of resolution (that is, true lines, not spectral lines, which can be counted as well as photographed), I do not think it absolutely necessary that we should doubt our eyesight until a count is made and compared with somebody else's count or measurement. In case an actual count is demanded as proof of resolution, we shall have to throw away all our supposed resolutions of *Nav. rhomboides*, *Sur. gemma*, *Pl. fasciola*, and many other common test-objects, and go to the trouble of verifying by count and measurement before we can trust our work. Observers who can be considered "experts" on the subject, differ as to whether a count is actually needed, or is not needed, as proof of resolution; it is a matter of individual judgment, and rules cannot well be enforced. At the time of my seeing the 19th band of the test-plate, I had no difficulty in counting a portion of the band, but did not undertake to count the whole of it, as it would have been very difficult, there being no resting-points for the eye; and the whole band would have had to be counted without stopping, which with the low power used ($\frac{1}{6}$th immersion) would have been impossible for me to have done. I am, very truly yours,

Edwin Bicknell.

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* 'American Naturalist' for Sept., 1870, p. 425; and 'Monthly Microscopical Journal,' November, 1870.

BOSTON, *Sept.* 30, 1870.

DEAR SIR,—In reply to your note of inquiry concerning my experience with Nobert's test-plate, I would say that I have examined the lines and witnessed their resolution on sundry occasions.

I have seen the lines of the 19th band satisfactorily resolved with a Powell and Lealand's $\frac{1}{16}$th under the skilful manipulation of my friend Dr. Woodward, at the Army Medical Museum, Washington, D.C. I have repeatedly seen the same equally well in this city with some of Tolles' excellent objectives; I have one which with its immersion front is a $\frac{1}{13}$th that shows the true lines of the 19th band. This is the highest power of Tolles' make with which I have seen the lines in question.

One year ago, at the Eleventh Triennial Exhibition of the Massachusetts Charitable Mechanic Association, the board of judges (of which I was one) held several meetings to test the relative and abstract merits of microscope objectives exhibited. A $\frac{1}{10}$th Amician objective made by Tolles, and owned by Mr. Bellis, of Waltham, was on exhibition, which with a B eye-piece clearly resolved the lines of the 19th band of Nobert's plate, not only to my own satisfaction, but also to the full satisfaction of others, among whom may be specified C. K. Stevens, Esq., of this city, whose experience on this subject has been quite extensive. It is proper here to observe that Mr. Stevens has been an intelligent amateur microscopist for more than twenty years, and has had large experience with Nobert's plates. He imported a twenty-band plate some fifteen years ago for his own use, and possessed the best objectives of the most renowned makers abroad and in this country. Mr. Spencer, of Canastota, N.Y., made for him seven first-class objectives, on orders without limiting the price. I mention these points, merely to show that Mr. S. is no novice, but experienced and competent to judge, notwithstanding his modesty and retirement has kept his name from extended publicity. As this $\frac{1}{10}$th objective was reported to have been in the hands of other microscopists of justly acknowledged eminence, who placed the 16th band "at high-water mark" in its performance, at my suggestion other competent observers were invited to test its qualities. Among those invited were Dr. B. A. Gould, ex-President of the American Association for the Advancement of Science; Dr. Walcott Gibbs, Rumford, Professor in Harvard College: both of these accomplished observers stated in unequivocal terms the satisfactory manner of the work in fully resolving the 19th band; Dr. Gould is now in South America, but Professor Gibbs permits the use of his letter on the occasion, which reads thus:—

DR. JOSIAH CURTIS, CAMBRIDGE, *Oct.* 18, 1869.

DEAR SIR,—In accordance with your request, conveyed to me through Mr. Stodder, I called this afternoon on Mr. Tolles. With a $\frac{1}{10}$th objective and a B eye-piece, I saw the 19th band of Nobert's test-plate distinctly resolved into lines without any lateral bands or false images whatever. . . . . Respectfully yours,

WALCOTT GIBBS.

Professor Gibbs did not count the lines, nor does he profess to have counted them, but that he *saw* the lines separated.

Yours truly,
MR. C. STODDER. JOSIAH CURTIS.

Extracts from a letter from Dr. F. A. P. Barnard, President of Columbia College, New York.

PRESIDENT'S ROOM, *Sept.* 27, 1870.

MY DEAR SIR,—Since the bands of Nobert's plate are so exceedingly narrow, it seems to me not extravagant to claim that when with a power not greater than a $\frac{1}{10}$th, one portion of the breadth of one of them is truly resolved, the whole breadth should be so likewise. If they were four or five times as broad, I should certainly regard Colonel Woodward's assumption as too sweeping. On the other hand, in my counts which I made with a filar micrometer in the spring of 1868, and which I described to you at the time, I found it extremely difficult to count the whole breadth of the higher bands, principally because the manipulation of the micrometer disturbed, sooner or later, the sharpness of the definition. I did not count the 19th band entire. My method was this—first by means of the coarser bands used as a stage micrometer, to obtain the value of the revolution of the screw of the filar micrometer with the given objective; then by bringing the higher band into the field, to count as long as I could, and to determine the values of the intervals between the lines counted, and thus by inference the number of lines to the inch from the reading of the screw. I actually counted from twenty to thirty lines—never more, I think, than thirty. As your letter suggests, Sulivant and Wormley counted, apparently, *for the purpose of verifying Nobert's statements in regard to the plate*, Nobert himself objected to one of Colonel Woodward's early photographs, that it could not represent the band which it was claimed to represent, because the number of lines in the entire band was too small. This is a legitimate criticism of a photograph, because the distance between the lines of this print would not indicate the distance between the lines ruled on the plate unless the enlargement should be exactly known—and about that there might be some reasonable doubt —but the question, whether the lines are resolved when the real distance between them is known, is settled the moment that the measured distance between any two such lines distinctly seen is found to accord exactly with the real distance. When, for instance, I found that the value by micrometer of twenty spaces on the 19th band as counted was exactly equal to the value by the same micrometer of ten spaces on the 9th band, I could not doubt that the 19th was resolved. I subsequently mounted the micrometer on a detached stand, so that turning the screw should not disturb the microscope, but my eye or my illumination failed me, and it did not improve upon the former performance, or even do so well.

I do not wish to be quoted as absolutely dissenting from Colonel Woodward's proposition, but I do hold that whether or not the whole breadth of a band is so resolved to be countable, the question whether *any part* of it is *actually* resolved is settled beyond doubt, when that part has been actually counted and the distances between the lines have been measured.

Truly yours,

CHARLES STODDER. F. A. P. BARNARD.

It is now desirable to examine Dr. Woodward's pretensions a little closer. It should be known if, or not, he always requires "the lines to be correctly counted from one edge of the band to the other" as proof that they are seen. Dr. W. often exhibits the resolution of the 19th band to his visitors. Does he first always count the lines? Or does he not rather first resolve them, and then sometimes count? If he takes the last course, then he depends on his experience in determining that the true lines are visible, and his observations are as trustworthy as mine, no more so. Two gentlemen who have witnessed the resolution both in Washington and Boston, agreed in saying that that in Boston was the best.

Has Dr. W. ever really "correctly counted the lines in any one band?" which in his "judgment" is the *sine quâ non.*

After all that he has written it would not seem that this question is yet to be answered, yet an analysis of his various reports and letters shows that there may be grave doubts.

In his letter of June 18th he says, "I supported my statement by a count of the lines, as well as by the photographs of Dr. Curtis." "I thought, and still think, that Dr. Curtis's photographs were conclusive evidence that I had seen the true lines, although the spurious lines shown on the edges of the band prevented them from serving for the purpose of a count. I have recently made myself another photograph of the same band, which will perhaps serve to convince any who are still incredulous."

In the 'Am. Jour. Sci.' for September, 1869, is Dr. W.'s original account of his first resolution of the 19th band. He writes, "I send two prints on glass, of which the first shows the 16th, 17th, and 18th bands satisfactorily resolved. The second shows satisfactorily the 19th band only. These pictures should be studied under a power of from two to six diameters. In counting the lines on them some doubt might arise, especially in the case of the 18th and 19th bands, as to the real number of lines; for certain spurious lines which are interference phenomena may be seen on the margin of the bands, and it is not always easy to tell which is the last real and which the first spurious line. A comparison of *several glass* positives from different negatives with each other, and with the *bands as seen in the microscope, where a change of focus materially aids in the determination, has led me to adopt the count above mentioned.*" Is this "correctly counting any one band from one edge to the other," as in his judgment is absolutely necessary? After all that comparison of photographs and microscope, he *adopted* a count, just the requisite number, and passes it as real.

I have copies of the photographs referred to, and I affirm that they do not admit of a correct count. The picture of the 19th band has over seventy lines, and it is a matter of fancy, or taste, or judgment, which shall be rejected as false, and which "adopted" as true. Anyone can "adopt" enough to leave the right number. Dr. Curtis's photographs do not give conclusive evidence that Dr. W. had ever seen the true lines; and they are not evidence that he had not seen them. How is it with Dr. W.'s own photograph? A copy of that is now before me. It purports to be a picture of the 19th band 1100 diameters, and a portion enlarged to 2800 diameters. The picture of 1100 diameters is an excellent picture of the 19th band as I have seen it, *not* resolved. Examined with a lens, an approximate count can be made with difficulty. There are certainly over sixty lines, and some of them on one edge are so coarse that they are evidently composed of two or more of the true lines. The enlarged portion is evidently made by stopping off on the negative the coarse lines of one edge and some of the finer ones of the other edge; not a difficult feat to leave the wished-for number, yet there are parts of the enlarged picture where I and others have been obliged to guess whether one or two lines should be counted. This is all that Dr. Woodward has offered, that he has "counted correctly one band from one edge to the other." Under the circumstances, considering the pertinacity with which Dr. Woodward has denied the claims of all others, I feel justified in being "still incredulous," as are many others.

Dr. Woodward has seen fit to introduce some extraneous matters which have nothing to do with the question whether I ever resolved the Nobert test-plate or not. As they have been published, I am under the necessity of noticing them.

He says, "Mr. Stodder writes with a warmth that will be best understood when it is known that he is the Treasurer and Agent of the Boston Optical Works, the establishment at which Mr. Tolles produces his really excellent lenses, and that he has for some time claimed that Mr. Tolles produces the very best lenses in the world. I am not willing to yield to either of those gentlemen in the disinterestedness of my desire for the success of American opticians, but am of the opinion that our progress will be hindered rather than helped, if we shut our eyes to the few cases in which English or other manufacturers excel ours."

I copy all the above that there may be no mistake in the language used. Dr. W., imitating his "witness," Dr. Hagen, must refer to my connection with the Boston Optical Works, of course wishing his readers to infer that my opinions are governed by interest. I have no reply to make to that.

I do not know by what authority Dr. W. says that I have made such a claim for Tolles' lenses; I have never published any such claim; when I do I shall refer to Dr. Woodward's own reports for evidence of the propriety of it; *e.g.* in the 'Am. Jour. Sci.'* he gives the results of his trials of various objectives of different powers, from $\frac{1}{8}$th to $\frac{1}{50}$th, and by various makers. No one could resolve the test-plate finer than the 15th band. This statement he repeats in the 'Quar. Jour. Mic. Sci.' for October, 1868.† In the 'Monthly Mic. Jour.'‡ he gives the following, additional:—"The $\frac{1}{8}$th of Wales and $\frac{1}{25}$th and $\frac{1}{50}$th of Powell and Lealand, all dry lenses, resolved the 15th band, and not the 16th. An immersion $\frac{1}{15}$th by Wales resolved the $\frac{1}{16}$th band, and failed to go farther. An immersion $\frac{1}{20}$th by Wales resolved the 17th band, but failed to go farther. A Hartnack, No. 11, also resolved the 17th band, and failed to go farther. A Tolles' immersion $\frac{1}{5}$th, recently constructed for Dr. J. C. Rives, of this city, resolved the 14th band." "A Tolles' immersion $\frac{1}{10}$th:§ with this I was unable to see beyond the 16th band." Dr. W. has broached this subject—not I. I now ask Dr. W. to name the "few cases," or any one case, in which the lenses of English or any other makers have excelled the performance of those two. Did he ever see, read of, or hear of any $\frac{1}{5}$th or $\frac{1}{8}$th objective of any other maker that had excelled what those did in his own hands? I ask him to reply without equivocation—yes or no—as publicly as he has provoked the inquiry.

Dr. Woodward takes pains to say, "Mr. Stodder's complaint that his paper in the 'American Naturalist' has been ignored, certainly cannot apply to me." Dr. W. has made the application himself; I did not have him in my mind when writing that passage; but as he has, uncalled for, entered a disclaimer, I will now specify that he has, as I believe intentionally, ignored the claims of Nachet, Mr. Greenleaf, and myself. On the 31st of May, 1869, Dr. Woodward gave a lecture before the Biological and Microscopical Section of the Academy of Natural Sciences of Philadelphia; a synopsis of the lecture is reported in the 'Dental Cosmos,' Philadelphia, August, 1869.* When he exhibited the photographs of the test-plate, he referred to his paper in the 'Microscopical Journal' for 1868, and is reported to have used the words: "At the time he published that article no microscopist had succeeded in seeing the true lines in any of the bands in this plate beyond the 15th." It seems to most people that this is "ignoring," and something more. But the Doctor may say that he is not responsible for the report. Well, let us see something else that he cannot deny responsibility for. About the time of the delivery of the lecture, and before that report was issued, he published Dr. Curtis's photographs of the claimed resolution of the 19th band, with two pages of letter-press explanation, a copy of which is now before me. The last two lines read, "These pictures of the Nobert's test-plate are the first photographic representations of this interesting test-object, and the true lines in the 16th, 17th, 18th, and 19th bands were first resolved at the Army Medical Museum."

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* Vol. xlvi., p. 353, Nov., 1868. † P. 229. ‡ Vol. ii., p. 292.

§ Dr. Woodward and Mr. W. S. Sulivant both say that the $\frac{1}{10}$th objective referred to is only an eighth, "English standard." The instrument has since been measured by two parties, one made it a full $\frac{1}{10}$th, the other a little less. The English standard and the American are the same, *viz.* the English inch. The mistake of those gentlemen was that they took for granted that their English objectives *must be* right. On this point I will copy from Dr. W. B. Carpenter, as I suppose that everyone has not read his book: "It may be well here to remark, that the designations given by opticians to their objectives are often far from representing their real focal length, as estimated by that of single lenses of equivalent magnifying power; a temptation to *underrate* them being afforded by the consideration, that if an objective of a certain focus will show a test-object as well as another of higher focus, the former is to be preferred. Thus it happens that what are sold as half-inch objectives are often more nearly $\frac{4}{10}$ths; and that what are sold as $\frac{1}{4}$ths are not unfrequently more nearly $\frac{1}{5}$ths" ('The Microscope.' London. 4th ed., p. 184).

This quotation shows the practice and explains the reason for it. See also 'Quar. Journ. Mic. Science,' 1862 and 1863; Mr. E. Bicknell in 'Am. Naturalist,' 1870; and Mr. Cross in 'Journ. Franklin Institute,' Philadelphia, 1870, and republished in 'Monthly Mic. Journ.,' Sept., 1870.

I am happy to say that at the recent meeting of the American Association for the Advancement of Science, held at Troy, New York, a committee was appointed by the suggestion of Dr. Josiah Curtis, of Boston, to consider this subject, and to secure, if possible, uniformity of designation at least for American objectives.

I will here take leave of the subject.

BOSTON, MASS., U.S.A., *Sept.* 30, 1870.

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* P. 397.

NEW BOOKS, WITH SHORT NOTICES.

A Report on the Microscopic Objects found in Cholera Evacuations. By Timothy Richards Lewis, M.B. Printed by order of Government. Calcutta: Office of Superintendent of Government Printing. 1870.—It is but a few years since great sensation was produced by Hallier's researches into the nature and cause of cholera. It was then pretty nearly believed that the cholera-fungus was really the cause of the disease, and that by avoiding connection with this plant the disease might be altogether avoided. But of late so much good work has been done on the subject of cholera-causation, that the German Professor's ideas have been shown to be extremely vague, and to be devoid of that care and precaution in their formation which are so essential in all experiments relating to the question, Whence comes cholera? We do not see that the author of the present volume had a very difficult task to perform in exposing the error and manifest carelessness of Professor Hallier's opinions. Still, it is perhaps as well that he should have done so, for so many people are prepared to believe anything stated on German authority, that the cholera-fungus may have yet many supporters, even in this country. But apart altogether from the several views which have from time to time been put forward on the cholera-fungus subject, this work has a special value. It is not that the author has committed himself to any view or endeavoured to support any particular theory; but from the circumstance that he has exposed somewhat fully the views of the fungus theorists, and has himself more fully than anyone else gone into the whole subject, and cultivated at considerable trouble, and with the most careful investigation, the various fungus bodies, and so far shown that the theories held upon the subject have really no foundation in point of fact.

The work is somewhat unsatisfactory in design, being rather a record of experiments than a continuous essay on the subject. Still, it is extremely valuable from the number of microscopic drawings (more than 100) which it contains, some of them being copies of those of other observers, but the great majority being the results of the author's inquiries into the subject. We notice that the powers employed are, generally speaking, very low; indeed, the highest, we think, employed is 600 diameters. We fancy that this was an error. Undoubtedly, with higher powers much more would have been observed; and we think it is to be regretted, for the work is the fullest and, from its illustrations, the most complete on the subject. The plan of the volume is somewhat vague, but the end is clearly attained, and shows us that we cannot hold any of the existing theories as to the cholera-fungus proposition. It by no means proves that cholera is not propagated by fungi; but it shows that the present theories have in fact no foundation. Here and there the author points out some remarkable facts, as, for example, the development of *Mucor* from *Penicillium;* but on the whole his inquiries are valuable but from one aspect, that of the cholera-theories. We thank Dr. Lewis for the publication of this work, for while it has involved immense labour upon the author, it tends to prevent us from assuming as a certainty that cholera is due to the fungus described by Hallier; and that cannot be held in the History of Science to be a matter of no importance. Dr. Lewis, we believe, proposes to continue his researches, and we hope he may, for good as his present work is, we may even expect a better labour in the future.

"*On a Searcher for Aplanatic Images applied to Microscopes, and its Effects in increasing Power and improving Definition.* By G. W. Royston-Pigott, M.A., M.D., Cantab., M.R.C.P., F.C.P.S., F.R.A.S., formerly Fellow of St. Peter's College, Cambridge. Communicated by Professor Stokes, Sec. R.S."—A paper on a new instrument having recently appeared in the 'Philosophical Transactions,' and been reprinted from them for private circulation, we think some notice of it will not be unwelcome to our Fellows.

The writer seems to have long felt, with other microscopists, that something remained to be done—first, in overcoming the great inconvenience attending the use of very deep eye-pieces, deep objectives dangerously close to the covered object; and next, in adding further corrections, in addition to those of the screw-collar for different thicknesses of glass cover. In these principal points he appears to have succeeded.

The traverse of a system of adjustable lenses between the fixed eye-piece and objective produces some remarkable changes in the definition, better or worse, according to the skill of the operator, and admitting a much wider range of chromatic and spherical corrections than can possibly be obtained by merely separating the objective lenses by means of the screw-collar. The author of this paper states (p. 598):—

"Using additional compensating lenses to gain increase of power, intermediately placed between eye-piece and objective, the finest definition is obtained when each of the three sets, *viz.* lenses, observing and image-objective, are similarly, though slightly over-corrected, as compared with a standard defining distance of 9 inches.*

"Although a fine definition seemed now attainable by means of supplementary compensating lenses, if judiciously introducing balancing compensations, yet their practical adjustments were innumerable and tediously accomplished.* At this stage of the research, frequent consideration of the well-known optical equations for a vanishing aberration fortunately suggested to me the idea of searching the axis † mechanically for aplanatic foci. In reference to these equations, which would be out of place here, it has been observed by Mr. Parkinson, F.R.S.,‡ 'If the aberration for rays parallel at incidence of a compound lens of given focal length, consisting of several thin lenses in contact, be examined, it will consist of a series of terms similar to that in Art. 129, one term for each lens, and the condition that the aberration shall vanish will lead to an equation involving more than one unknown quantity, and consequently admitting an unlimited number of solutions.' "

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* "It is convenient to define the aberration to be positive or negative, or the lens to be *over* or *under corrected*, by the simple fact that an ordinary lens causes the excentrical rays to cross the axis at a point nearer the centre of the lens than the centrical rays, in which case, and in all analogous cases, it may be said that the lens is under-corrected and afflicted with a negative aberration. All the best objectives are now constructed on the principle of having the posterior sets over-corrected and the anterior under-corrected so skilfully as to destroy by opposite errors nearly the residuary aberration; but the opinion may be hazarded that future combinations will yet be found which will completely throw into the shade the present powers of the microscope, when, perhaps, we shall be in a better position to attempt to determine the microscopical features of molecular life, at present probably beyond its grasp, as no single particle so small as the $\frac{1}{80000}$th of an inch in diameter can be clearly defined, if isolated, until residuary error is very much more reduced.

"It is to be regretted that the precise nature of the marvellous combinations invented by Professor Amici for objectives remains unknown. As one of the Jurors in the Paris Exposition, his microscope necessarily remained both uncelebrated and unelucidated in the Reports.

Mr. Lister, whose paper in the 'Phil. Trans.,' 1830, accomplished so much for the development of the modern microscope, though no mathematician, ascertained the positions of two aplanatic foci, the longer and shorter; but it would appear from this research that a great number of aplanatic foci exist at different points of the axis. And that whilst many objectives refuse to define well at the standard distance a variation of the distance, as attained by the new instrument, the aplanatic searcher, may be rewarded with success; especially when it is considered that the searcher automatically changes its own aberrations by a simple traverse. One of the advantages attending its use is thus described:—

"When it is desirable to view an object through a very thick refracting medium, the searcher is brought as close as possible to the objective, which action lengthens the focus of the objective; and the same thing is necessary when the observer wishes to throw the *eidola* of an upper structure above and away from the true image of the lower but contiguous stratum—as when the lower beads of the Podura are required, or when it is required to give additional negative aberration to an objective too positively corrected in which the front glasses are already in dangerous proximity."

"On the contrary, when the searcher is traversed the opposite way the *objective lenses* require to be brought nearer together; the instrument is then more adapted for viewing objects or particles lying in the upper plane of a complex structure, throwing the *eidola* of the lower layer below that layer itself, and so leaving the upper stratum less disguised by the false images of the lower."

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* "During 1865–1869 many experiments were tried with complete objectives and various parts of them, either over or under corrected, by means of a sliding tube carrying them and fitting into the 'draw-tube.'

"Professor Listing, of Bonn, more recently has in two papers confirmed the value of this method of amplification quite independently. Nachr. d. hgl. Gesell. der Wissen, 1869, No. 1, and Poggend Annalen, 1869, t. 16, p. 467 ('Nature,' Jan. 27, 1870).

"In the first he recommended an inverted Huyghenian eye-piece, and in the second intermediate achromatic lenses.

"As regards intermediate lenses, the writer has ascertained (Nov. 1870) that Dr. Goring ('Micrographia,' ed. 1837) has anticipated both these methods.—Note added Nov. 1870."

† This term "searching the axis" was early employed by Dr. Pigott, *viz.* in his first essay, received May 21, 1869. He says, "A search for the real focus or best image should not be neglected along the axis of the instrument" (p. 297, Dec. No. 1869).

‡ "Griffin's 'Optics,' by Parkinson, p. 122."

"In intermediate cases, where greater penetration or focal perspective is required, with a thin glass cover, the objective lenses must be proportionately separated by an increased interval, the searcher being traversed towards the objective; and in general confused images of both upper and lower strata can be obtained by opposite arrangements.*

"A very interesting refinement upon the corrections for chromatic effects may be accomplished by gradually traversing either way both searching and objective lenses and closely watching the effect.

"The most brilliant definition is generally obtained when the searcher (a little more over-corrected) is used as close to the objective as possible.

"The over-correction of the searcher is increased by separating its component lenses according to the divisions upon the sliding tubes of the searcher.

"The change in the general aberration is shown by the divided index of the milled head actuating the movement of the searcher.

"The power obtained is in general from two and a half to four times greater than that given with the third eye-piece C of 1-inch focal length: with a very fine eighth of Messrs. Powell and Lealand's *new* construction, a clear and satisfactory definition of the beading of the *Pleurosigma formosum* was exhibited to them, by means of the aplanatic searcher, at a power *estimated* at 4000 diameters.†

"This paper, perhaps, will hardly be complete if I omit to add, that the instrument will be most effectively employed by considering it as a conjugate portion or integral part of the objective itself, in which the minute traversing adjustment of the objective lenses finds its counterpart in the more extended and therefore more delicate adjusting traverse of the searcher itself. So that, in short, during minute microscopical research each adjustment should be intelligently applied, according to the nature of the research in hand. The indications of the one adjustment should be employed to verify those of the other. Correlative movements by the aid of the searcher may introduce aplanatic images, whilst a violation of their correlation will exhibit deformity."

It would appear from this paper, that the very finest definition and working power is to be sought rather in the use of comparatively low objectives, such as the $\frac{1}{8}$th, $\frac{1}{5}$th, and $\frac{1}{4}$th. In a note the author states (p. 602):—"That a Wray $\frac{1}{5}$th made expressly, admitted of as great amplification as an ordinary $\frac{1}{12}$th. In fact, these researches appear to point decisively to greater advantages to be expected from raising the quality of the lower objectives rather than deepening focal length. Observers are more numerous every year who prefer the $\frac{1}{8}$th to the $\frac{1}{25}$th and $\frac{1}{50}$th."

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* "Such as separating the objective lenses and traversing the searcher farther from them.

† "With a 'Kelner' two-thirds of an inch focal length, a very clear, very large and flat field is presented to the eye, notwithstanding the increased power with the searcher. A 1½-inch objective by Ross was used generally for a condensing illuminating apparatus more or less stopped off. "The usual power of the $\frac{1}{8}$th with a C eye-piece is 800; a power of 4000 is given by an eye-piece of one-fifth of an inch focal length."

"APPENDIX.

"The law of displacement followed by the final focal image corresponding to a minute displacement of the internal lenses of a complex objective, the front lenses or facet *remaining fixed*, possesses some interest and may thus be expressed:—

"Let F be the distance of the final focal image when the objective lenses are closed together.

"$F + \delta F$ its distance when the front sets of the objective are displaced by a quantity δx.

"Then it will be found if f_1 be the distance of the virtual image conjugate with the *object* as formed by the front set of lenses,

$$\delta F : \delta x :: -F^2 : f_1^2 ;$$

and consequently every slight change of the screw-collar of an adjusting objective produces comparatively a very large displacement in the final focal image, and therefore of the traversing image searcher; so that the searcher-traverse represents a movement conjugate with the objective index. Again, since this traverse towards the objective encounters rays of increasing divergence, an increasing breadth of pencil is encountered by the lenses of the searcher, and its own peculiar aberration receives an instantaneous increase, which introduces an important new element in definition; it having been observed that the glasses must be very gradually over-corrected as the image is formed nearer the objective."

The rage for diatom resolving microscopes requiring an enormous angular aperture will now probably be abated in favour of instruments long preferred by working naturalists, to whom a just display of an object in relation to its parts, by penetration or depth of focal perspective, with a working distance between the object and objective sufficiently roomy to admit manipulation, are conditions indispensable to organic structural research. We cannot but think Dr. Royston-Pigott's invention a valuable one, even if it does nothing more than substitute low eye-pieces for deep ones, and enable the half-inch power to work at a greater distance, with a power of 800. We observe with pleasure the paper was communicated by Professor Stokes, whose name has so long been associated with advanced optical science.

We think it should be here intimated that Dr. Royston-Pigott has tried a variety of convex and concave lenses for searchers. The "Barlow" achromatic concave lens, when not made too deep, produces some good effects, gives an inverted image instead of erect, but also often displays a "central ghost," and is incapable of producing the automatic and differential adjustments so much prized in transcendental definition. There will doubtless be many cheap imitations, which, it may be feared, will only caricature the properly-constructed instrument, which must be regarded as a new aid to microscopical research.

PROGRESS OF MICROSCOPICAL SCIENCE.

Metamorphoses of Siredon Mexicanus.—Professor E. D. Cope has a short but interesting paper on this subject in Silliman's 'American Journal' for February. He states that Professor Baird was well acquainted with the metamorphoses of this group at the time that they were observed in the Jardin des Plantes. Even so early as 1847, in his essay, he regarded several species as larvæ. He, after some general observations, records his belief that *S. Mexicanus* will be found to undergo metamorphoses sometimes, but he states that he has not observed it.

Insects in Deep Salt-water.—Mr. A. S. Packard, jun., gives in the above-named journal a notice of certain insects obtained from deep sea-water by Professor Verrill. Some of them are microscopic, and it is puzzling enough to imagine how air-breathing animals obtain air at 20 fathoms depth in salt water. The author says the present species was dredged by Professor Verrill in 20 fathoms, on Clark's Ledge, in Eastport Harbour. It was found (four or five specimens, young and adult) "on hydroids," &c. It will be an interesting point to determine whether, like the other species of the genus, it also lives in the earlier or even in the adult state among the gills of Lamellibranchs, and also whether it lives between tide marks, thus agreeing with the distribution of *Chironomus oceanicus.* At any rate we have here an insect and a mite breathing by tracheæ, and extracting the oxygen from the water at the great depth of 120 feet, and, in the case of the dipterous larva, with no apparent variation from specimens living at low-water mark. In this connection he notices the fact that they have on the New England and Labrador shores several species of mites of the family Trombididæ, which run over seaweeds and live under stones between tide marks, and he has observed similar species at Beaufort, N.C., and Key West, Florida. As regards the distribution of the species of brine insects, several questions of interest arise. How are we to account for the origin of the *Ephydra halophila* in such prodigious quantities in the vats of the Equality Salt Works of Illinois, a locality remote from salt lakes and the ocean shores? Are the brine species of the Salt Lakes of Utah and California remnants of an oceanic fauna and of the tertiary period, or are they of recent and local origin? Have these brine insects acquired their singular tastes within a recent geological period (say the Quaternary), having lived at first as do their allied species, in foul fresh-water, or amid decaying matter in damp localities? Before these and other questions can be answered, we must have analyses of the waters, and a review of the European literature on the subject, and larger collections of brine animals from our own country.

Infusoria and Sponges.—It does not at first appear that there is a relationship of an intimate nature between these two. Professor H. James-Clark, of the Kentucky University, appears to think differently however, and imagines that there is an intimate relationship between the two classes. He gives the following account of an intermediate form. He says that in Schultze's 'Archiv. für Mikroskopische Anatomie,'* Cienkowsky describes, under the name *Phalansterium*, a genus which consists of monad-like bodies with a *flagellum* and a projecting collar like those of *Codosiga*, *Salpingœca* and *Leucosolenia*. Of the two species which he illustrates, one (*P. consociatum*) has monads enveloped in a broad funnel-shaped, slimy sheath, and these sheaths are closely packed side by side, radiatingly, so as to form a shield-like or a hemispherical mass. This comes nearest to the *Salpingœca*. The other species (*P. intestinum*) possesses similar monads, but they are imbedded basally in a gelatinous, intestiniform mass of slime (*Schleim*), "with their vibrating lashes extending in every direction" about the cylindrical colony. Originally each monad is endowed with a separate slime-sheath; but eventually these all are fused together into one common mass. Beyond this, to make a true Sponge we need but the presence of spiculæ, and open interspaces in the slimy mass, between the monads, leading to one common cavity. Introvert the layer of monads, and we produce the desired effect without doing violence to their relative positions. It is a mere matter of proportions, just as the inverted cyathiform rose-hip is none the less an ovariferous disk than the globular receptacle of the strawberry.

Striated Muscular Fibre in Gasteropoda.—Mr. W. H. Dall (of America) says that in studying the radula of a species of *Acmæa* (probably *A. Borneënsis*, Rve), obtained by Professor A. S. Bickmore at Amboyna, he noticed, on placing the structure under a power of 100 diameters, that certain of the muscular fibres which adhered to it when torn from the buccal mass, had a different appearance from the others. On increasing the power to some 800 diameters, it was at once evident that the different aspect of these fasciculi was caused by fine, but clearly defined, transverse striation. Suspecting that it was an optical delusion, caused by a very regular arrangement of the nuclei of the fibres, he subjected the muscle to various tests and to still higher magnifying powers. He also introduced under the same glass, some of the voluntary dorsal muscles of a small crustacean, for comparison. The structure of the ultimate fibres in both appeared to be similar. These seemed to be composed of a homogeneous tube or cylindrical band of translucent matter, with nuclei interspersed at irregular intervals. In neither was there any appearance of separation into transverse disks, as is seen in the striated muscles of vertebrates. That the striated appearance was not due to contraction and folding of the muscle was evident upon taking a side view of one of the fibres, when the striæ on each side, as well as the intervening elevations, were seen to correspond exactly to each other. The only perceptible differences between the muscles of the crustacean and the striated muscles of the mollusk, appeared to be that the latter were much more finely striated; the striæ being six to eight times as numerous as in the former, in the same space. No difference between the striated and non-striated muscles of the *Acmæa* could be observed, except in the fact of the striation. In both the nuclei were irregularly distributed. The appearance of the striated fibre reminded one of a string of rhombic heads, which bore no relation to the position of the true nuclei. The striated fibres appeared, after a careful dissection of the parts in a number of specimens, to be the retractors of the radula; they were longer and in narrower bands than the non-striated fibres, and comparatively much fewer in number. The striation was most evident towards the middle of the fibres, and became evanescent towards their extremities.

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* Bd. vi., 4, 1870.

Are the Brachiopods Annelids?—Mr. Edward S. Morse replies at some length to the views of Mr. Wm. H. Dall on the above subject, and as his remarks extend to some length, but are of great interest, we give them bodily to our readers. "Mr. Dall says that the 'utmost impartiality' should be used in the discussion, and 'due consideration should be given to the facts,' &c. This 'utmost impartiality' is illustrated, by passing over in absolute silence, not only all my references to the limited knowledge we possess of the embryology of the Brachiopods as shown by Lacaze-Duthiers, but the dorsal and ventral plates—the serial arrangement above and below of setæ, and even the gill laminæ in *Lingula*, as first recognized by Savigny in the Annelids—the bi-lobed lophophore—the cephalic collar—the thin and muscular visceral walls—the singular results obtained by Gratiolet in the chemical analysis of the shell of *Lingula anatina*—the presence of the cæcal prolongations of the mantle, and their relations to the *porenkanale* in the Annelids—and, above all, the remarkable existence of one or more pairs of *segmental organs*, in form, character, and functions like the segmental organs of the Annelids, the first feature that led me to regard the Brachiopods as Annelids six months before I ever saw living *Lingulæ*. All these points are passed over in the 'utmost impartiality' of silence. Overlooking with the same impartiality my statement that the Brachiopods presented a *comprehensive type*, and comprised certain crustacean characters, he neglects to mention the winter egg of the Polyzoa, and similar features in the lower Crustacea, and we might add similar features in certain Rotifers, now admitted to be worms. So also the presence of striated muscular fibre in certain muscles of the Brachiopoda, their absence in the Mollusca (?), and their presence as one of the prominent characters in Crustacea. He gives us, however, Clark's definition of the Mollusca, which comprise the characters of the branch as then understood, which includes, of course, the Tunicata, Polyzoa, and Brachiopoda. In defining the Vermes, with the same impartiality, reference is made only to those Vermes in which the body is made up of a repetition of similar parts, overlooking entirely the unsegmental Vermes which comprise a large proportion of the class; nor is reference made to the remarkable cephalization of many Annelids, where the posterior portion of the body has been called a caudal appendage, being without bristles. In some, the thoracic rings, few in number, have a wide flaring membrane running continuously along each side (*Protula*). We leave others to judge of his conclusions respecting the character of the setæ, as also his startling homology of the peduncle of *Lingula*, and the siphonal tubes of a clam (!), and call attention to his statement, that because the setæ are not found the entire length of the peduncle, therefore they are by no means 'identical' with those of the worms. This will be a new idea to naturalists, that the confinement of appendages to limited portions of the body forbids all homology with those in which the appendages run the entire length. It indicates also that Mr. Dall believes that all Chætopods have setæ from head to tail. He states as a grave objection that the Brachiopods are invariably attached by muscles to a bivalve shell. In the same breath he should have added that worms are also invariably attached to a *bivalve* or multivalve shell, whether it be the scuta of *Sternaspis*, the oval plates of *Lepidonotus*, or the hardened integuments of others. He lays great stress on the presence of bristles in Chiton, as if that group were the very embodiment of the Molluscan type; but with the same impartiality he neglects to mention its embryology, so remarkably articulate, as shown by Lovén, its dorsal vessel, the double and forward opening of the oviducts, the anus terminal, and other features, so remarkably articulate as to induce De Blainville to recognize their affinities with the Annelids, to prompt Milne-Edwards to call them a Satellite group, and to cause Jeffries to liken them, in their different stages, to 'Isopods, tiny trilobites, Onisci and Aphrodita.' In mentioning my anatomical drawing of various Brachiopods, he says, 'Some of them taken from life;' he should have said, 'Most of them taken from life.' In concluding his paper, he refers to drawings in my collection of a 'singular sipunculoid worm,' 'and appears from them to have an anterior termination to the intestine, thus forming a notable exception to the general rule among worms.' The drawing to which he refers is the common sipunculoid worm of the coast. *Phascolosoma*, *Sipunculus*, and its allies claim it as a right to have an anterior termination to the intestine, a fact known to everyone who ever made them a study. Finally, in his comparisons, with the exception of Chiton, he confines himself to and only points out a few of the many relationships between the Brachiopods and Polyzoa and Tunicates, admitted by all. Now, since Leuckart, Gegenbaur, Haeckel, and other eminent naturalists of Europe, have seen conclusive reasons to remove the Polyzoa and Tunicates entirely from the Mollusca, and place them among the Vermes, it seems that Mr. Dall's first task should be to whip these back to the Mollusca before commencing with the Brachiopods."

Linnæus, the Originator of the Hypothesis of the Derivation of Species.—Ludwig von Hohenbühel-Heufler contributes an article to the 'Botanische Zeitung' for September 9, 1870, in which he claims for Linnæus the origination of *the* theory of the derivation of species, founding the claim upon the paragraphs which Linnæus appended to the sixth edition of his 'Genera Plantarum,' published in 1764, *viz.*:—

1. "Creator T. O. in primordio vestiit Vegetabile *Medullare* principiis constitutivis diversi *Corticalis*, unde tot difformia individua, quot *Ordines* Naturales, prognata.
2. "*Classicas* has (1) plantas Omnipotens miscuit inter se, unde tot *Genera* ordinum, quot inde plantæ.

3. "*Genericas* has (2) miscuit Natura, unde tot *species* congeneres, quot hodie existunt.
4. "*Species* has (3) miscuit Casus, unde totidem, quot passim occurrunt, *Varietates.*
5. "Suadent hæc (1–4) *Creatoris* leges a simplicibus ad composita. *Naturæ* leges generationis in hybridis. *Hominis* leges ex observatis a posteriori."

Whatever meaning be put upon these somewhat enigmatical propositions, they certainly show that Linnæus (as a naturalist of his turn of mind was not unlikely to do) had thought of a derivative origin of species as not improbable, and had formed some idea of an hypothesis concerning it,—perhaps as definite as his idea of natural orders,—a problem he could suggest rather than solve. And it is interesting to note the scale of operating powers,—Chance sufficing for the production of varieties, Nature for genera, the Omnipotent directly for the mightier work of producing orders. But in a subsequent number of the same journal (November, 1870), the veteran Professor Von Mohl maintains that Linnæus did not hold the theory of the origin of species which is now becoming so general.

NOTES AND MEMORANDA.

Alteration of Day of Meeting of the Society.—The next meeting of the Society will take place on the first day of March. In future, the meetings will be held on the *first*, instead of the *second*, Wednesday in the month.

Mr. Lee and the Croydon Microscopical Club.—Mr. Henry Lee, the President and originator of the Croydon Microscopical Club, delivered his annual address before a large and important meeting, on Wednesday, the 18th day of January last. After the usual business had been gone through, Mr. Lee addressed the members at some length. After dwelling upon the advantages of such clubs, upon the field that lay before them, and upon the success of the Society's soirée, he concluded as follows: — And, now, I come to a point of great importance in its influence upon the usefulness of our studies to ourselves and others: I refer to the systematizing of our work. I am satisfied with what has been done during our first year, but it is time that each of us began, in conjunction with others of similar taste, to follow up with a distinct purpose some particular subject. The purpose should be the more complete knowledge of our local natural history; and the subjects which I would especially indicate to you as not difficult, are entomology, botany, and microscopic palæontology and pond-life. On a former occasion I communicated to you my friend Mr. Wilson Saunders's suggestion that we should unite with the Holmesdale Club at Reigate in the botanical section; and I shall shortly ask those who are interested and able to help in the four subjects I have named, to confer with me respecting the organization of co-operative work upon them. I have seen so much skill and ingenuity displayed by many of our members in preparing and mounting objects, and so much latent talent in this direction, that I have determined to offer a prize, to be presented at our next annual meeting, for the best series of five dozen mounted objects. The rarity, novelty, and scientific value of the specimens, difficulty of preparation, and neatness of manipulation will be duly considered. The competitors must be amateurs and members of the club, the judges will be two members of the council of the Royal Microscopical Society, and the prize will be a cabinet designed by myself to contain all the necessary apparatus for the mounting of microscopic objects, and of the value of 5*l*. I conclude with the assurance of the happiness I have experienced in associating with you all during the past year, and with the hope that, encouraged and assisted by our fellow-members, and stimulated by their approbation and example, we may be able, during that which is to come, to maintain and increase the attractiveness and usefulness of our club.

How to Mount Objects.—A good paper on this subject appears in the 'Journal of the Quekett Club' (January, 1871). It is by Mr. D. E. Goddard. It is too long for abstract, but we refer to it, as some of our younger readers may find parts of it extremely beneficial. In 1863, Mr. Goddard described, in the 'Quarterly Journal of Microscopical Science,' a peculiar table for mounting objects, which has also an interest for the student in connection with the present subject.

Spontaneous Generation.—Dr. Bastian's ideas are well opposed by Mr. T. B. Lowne in a paper "On Spontaneous Generation," published in the 'Journal of the Quekett Club.'

A Neutral-Tint Selenite Stage.—Mr. W. Ackland describes, in the 'Journal of the Quekett Club' for January, a useful form of selenite stage, which gives a tint between the violet of the second, and indigo of the third, wave. He shows how this can be done, and describes some excellent results.

CORRESPONDENCE.

A Committee for Examination of Objectives.

To the Editor of the 'Monthly Microscopical Journal.'

2, Lansdowne Crescent, W.

Dear Sir,—It would give me great pleasure to hear of a Committee being formed for the Examination of the relative qualities and powers of English and Foreign Microscopic Objectives, whether *dry* or *immersed*.

[In order to prevent all suspicion of prejudice, it would perhaps be advisable for each maker to mount the glasses in similar plane cylindrical forms with screw-collars, the pattern of which should be previously assigned, and identification secured either by a small engraved motto or high numbers. The motto with the maker's name to be enclosed in a sealed envelope, to be opened by the committee after its final award. The definition to be tried both with full and reduced apertures, with long and short tubes and *approved* eye-pieces (deep), C, D, E, F of ascertained focal length.]

(1) For Resolution.
(2) For Penetration or depth of focus.
(3) For Magnifying Power.
(4) For Spherical and Chromatic Aberration.
(5) For Angular Aperture.

Illumination to be entirely *centrical* through a 1½-inch objective condenser, limited to 10° aperture, used obliquely or directly.

I cannot but think that some such scheme would greatly benefit microscopical science, and settle some necessary *axioms* about which we are very much at sea at the present moment.

I am yours, very faithfully,

G. W. Royston-Pigott.

P.S.—I propose at the next meeting to exhibit and illustrate the little instruments alluded to in this number, should the President approve of this suggestion.

English Microscopists.

To the Editor of the 'Monthly Microscopical Journal.'

4, St. Martin's Place, *Feb.* 16, 1871.

Sir,—You have done me the honour of indirectly referring, in your January number, to certain experiments of mine with infusions of milk. If you had not done so, I should not have noticed the criticisms of the anonymous writer in the American journal from which you quote.

I quite agree with my critic that *amœba* is "an object about the nature of which very little is known," but I trust I am not so ignorant as not to know *amœba* when it shows itself under the microscope. Nor is it likely that I should mistake for infusoria the so-called oil globules of milk, the movement of which I have watched too often to be thus deceived. It appears to me to be very objectionable for writers to thus broadly, and in reality without the slightest ground, endeavour to throw discredit on the observations of others. Nor is it good taste, to say the least, to assume that all observations but those which agree with the notions of American microscopists are made with "inferior instruments," as compared with Mr. Holman's "*good* objective." The italics are *not* mine.

I am, sir, yours obediently,

C. Staniland Wake.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *February* 8, 1871.

The Annual General Meeting of the Society was held; James Glaisher, Esq., F.R.S., V.P., in the chair.

The minutes of the last meeting were read by the Secretary.

Mr. Ingpen said that before the confirmation of the minutes he would move that the minute in reference to the nomination paper sent in by Mr. Ince be expunged. By the 44th bye-law he believed that nomination paper was informal, inasmuch as it was not signed by three or more Fellows before it was brought before the meeting. If it had been brought before the notice of the meeting for the purpose of having the names of any two Fellows added to it, by the 44th bye-law it should have remained open until the close of the meeting. Mr. Ingpen cited the bye-law, and said it seemed to him that as the notice stood upon the minutes it was nothing more nor less than an intended slur upon one of the Society's oldest members. On this account he moved that it be expunged from the minutes.

Mr. J. Charters White seconded the motion.

The Secretary explained that no Fellow was absolutely interdicted by the bye-laws from bringing forward a motion without notice, except one for altering any bye-law. The notice was no doubt informal, but out of courtesy to the gentleman who handed it in, the Chairman put it to the meeting.

The Chairman was willing to take some blame to himself for reading from the chair a notice which was not in strict accordance with the rules of the Society; but the paper having been put into his hands after he had taken the chair, out of courtesy to the gentleman who proposed the motion he had read it. He apologized for the infringement of the rules in the manner which had been indicated.

The proposition was then put to the meeting that the minute relating to the nomination paper handed in by Mr. Ince should be expunged from the records of the Society, and agreed to by a large majority.

The list of donations was then read, and the thanks of the meeting presented to the respective donors.

Mr. Lee said some months ago he had the pleasure of mentioning the fact that a friend had authorized him to spend 20*l.* in the purchase of objects for the Society's cabinet. He had had it in contemplation to purchase Mr. Carter's bone sections, but Mr. Joseph Beck's handsome present had forestalled his purpose. Subsequently some objects came into his possession collected by Mrs. Clarke, of Whitby, and he now had to announce that 210 specimens of the Marine Algæ of Whitby had been presented by Mr. H. Whitling as a first instalment.

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* Secretaries of Societies will greatly oblige us by writing their reports legibly—especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. M. M. J.

A special vote of thanks was given to Mr. Whitling for this valuable donation, and thanks to Mr. Lee, through whom the present had been made.

The President then appointed Messrs. Hilton and Suffolk to act as Scrutineers of the ballot for the election of the officers of the Society for the ensuing year.

The Chairman announced that he had received a letter from Dr. Royston-Pigott stating that an error had been inadvertently made in the engraving of the woodcut on page 66 of the Journal.[*] The error would be rectified in the next number.

The Secretary then read the financial statement for the year.

The Secretaries announced that the lamented illness and death of the late President had prevented his taking any part in the preparation of the Annual Address. Under these circumstances they had compiled an account of the Society's operations during the past year, and its present condition, to which Mr. Slack had added a *résumé* of some recent investigations with minute organisms. The Society was indebted to Dr. Millar for preparing an Obituary Notice of the late President. As Dr. Millar had some further additions to make, this and similar notices would be taken as read, and would appear in the next number of the Journal.

At the conclusion of the Address, Mr. Glaisher then moved that a special vote of thanks be given to Mr. Slack for the admirable Address he had given, undertaken at very short notice in consequence of the lamented event before referred to.

The motion was unanimously agreed to; and it was also resolved that the Address be printed in the usual way.

The Chairman having announced that the Scrutineers had reported that the officers, as proposed by the Council, had been unanimously elected, the meeting was adjourned to the 1*st March next*, the alteration in the day of meeting commencing from this time.

Donations to the Library and Cabinet, from January 11th to February 8th, 1871:—

	From
Land and Water. Weekly	*Editor.*
Nature. Weekly	*Editor.*
Society of Arts Journal. Weekly	*Society.*
Athenæum. Weekly	*W. W. R.*
Journal of the London Institution, Nos. 1 and 2	*Institution.*
On a New Genus of Graptolites. By John Hopkinson, F.G.S.	*Author.*
The Annual Report of the Brighton and Sussex Natural History Society	*Society.*
Notes of some of the Researches in Anatomy, Botany, &c. By George Gulliver, F.R.S.	*Author.*
J. D. Möller's Diatomaceen-probe-platte	*The Rev. R. N. Browne.*
Six Slides of Copper Ore, &c.	*The Rev. R. N. Browne.*
Two hundred and ten Slides of Marine Algæ	*H. T. Whitling, Esq.*

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[*] The Editor is by no means responsible for this error.

The following were elected Fellows of the Society:—

William Alfred Bevington, Esq.
William Ackland, Esq., L.S.A.
Charles Mostyn, Esq.
H. R. Webb, Esq.

Walter W. Reeves,
Assist.-Secretary.

RICHARD MESTAYER, TREASURER, IN ACCOUNT WITH THE ROYAL MICROSCOPICAL SOCIETY.

1870.	£.	s.	d.	1870.	£.	s.	d.
Balance brought from last year	28	0	3	Hardwicke for Journal	260	6	6
Subscriptions for 1867	1	1	0	Insurance	1	4	0
" 1868	8	8	0	King's College, rent, &c.	50	0	0
" 1869	32	11	0	" " refreshments	29	11	9
" 1870	333	18	0	" " soirée, 1870	18	19	6
" 1871	3	3	0	Instruments and repairs	5	3	0
Admission fees	31	10	0	Cabinet-maker	5	5	6
Compositions	56	14	0	Books	7	14	6
Dividends on 1059*l.* 6*s.* 2*d.* Consols	31	3	8	Stationery and printing	11	10	9
Transferred from the Charter Fund	35	0	0	Petty cash	24	4	10
				Assistant-Secretary	63	0	0
				Reporter	9	9	0
				Diagrams	2	0	0
				Debt due from the late Collector	38	17	0
					527	6	4
				Balance due from Treasurer, 31st Dec., 1870	34	2	7
	£561	8	11		£561	8	11

Examined and found correct,

W. T. SUFFOLK, } *Auditors.*
JAMES HILTON, }

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

November 24th.—Microscopical Meeting. Mr. Glaisyer, Vice-President, in the chair.

Mr. R. Glaisyer reported the receipt for the Society's cabinet from Mr. Hennah of three slides of tooth sections, made by the late Mr. T. Eden, and of two slides from Mr. Wonfor.

Mr. Dennant reported that for the grinding sections of teeth he had found Water-of-Ayr stone, used in the same way as pumice stone with ground glass, gave better results than the pumice stone. He considered the operation quicker, and that there were fewer scratches. He should like to know the best means of staining teeth or bone.

Dr. Hallifax recommended carmine; but Mr. Hennah preferred magenta, a drop or two in water was quite sufficient.

In answer to an inquiry from Mr. Peto as to the office of the *lacunæ* and *canaliculi* in bone, and the supposed cause of the difference in their size, Dr. Hallifax described the vertebrate skeleton and its structure, pointed out its power of repairing injury, its special purposes of leverage, &c., and discussed the *haversian* system, the *lacunæ* and *canaliculi*, all of which he considered subservient to the purposes of nutrition, and mentioned that by the process known as decalcification he had been able to make three sections with his cutting instrument, in which the haversian system was well illustrated.

Mr. Hennah stated that in the opinion of some authorities the *lacunæ* and *canaliculi* differed in size, according to the relative sizes of the blood globules.

Mr. Wonfor thought this would hardly hold good in all cases, for in some of the Reptilia, having large blood globules, the lacunæ and canaliculi were small.

The meeting then became a conversazione for the exhibition of specimens of "Bone and Allied Structures."

Mr. Hennah exhibited cartilage dry and in gelatine, and sections of bone in balsam.

Mr. Dennant exhibited sections of teeth showing ramifications of *caries*, and sections of bone.

Mr. Peto exhibited sections of bone, recent and fossil, and of teeth mounted in balsam.

Mr. Sewell exhibited human fœtal and other bone sections.

Mr. Wonfor exhibited sections of bone, bony scales, ivory, teeth and palate teeth, dry and in balsam.

It was determined that the Microscopical Meeting for Dec. 22nd should be on "Shell Structure."

December 8th.—Ordinary Meeting. Mr. Sewell, Vice-President, in the chair.

Mr. H. Pratt was elected an ordinary member.

Mr. Wonfor, Hon. Sec., announced the receipt (from the author) of a copy of a paper read before the Eastbourne Natural History Society, by Mr. F. C. S. Roper, "On the Polyzoa found at Eastbourne."

The Rev. J. C. Walker exhibited a curious flower, commonly called "The Rose of Jericho," and which, when placed in water, unfolded itself. This property is possessed by several plants, *viz.* the true Rose of Jericho, which belongs to another family; by one of the Lycopodiums, sold under the name of the Resurrection Plant; and by some of the lichens, as well as by the spores and seed-vessels of some plants, notably in the case of Erodiums, one of which has been used as a hygroscope, owing to their being acted upon by changes of moisture and dryness. An example of the Lycopods, *L. lepidophyllum*, possessing this property, was shown by Mr. Wonfor. The difference between this plant and the Roses of Jericho is, that, after being placed in water for twenty-four hours, the plant may be potted, and will grow like any other lycopod.

Mr. R. Glaisyer exhibited specimens of lias, from Dudley, containing fossils, among which was a perfect trilobite.

Mr. Nash exhibited shale, from which the petroleum oil is extracted, and containing fossil fish, the bones of which were distinctly seen; crude petroleum; petroleum scales, or the solid extract; purified wax, and manufactured articles from the same, illustrating the petroleum from its crude to its manufactured form.

Mr. Hennah exhibited some very beautiful specimens of arsenite of copper, collected by his grandfather from Cornish mines.

Mr. Wonfor exhibited three birds, lent by Messrs. Pratt—the Iceland gull, a very rare bird in Sussex, shot recently off Brighton by Mr. Goldsmid; the shore lark, an occasional visitor; and the grey phalerope, which had been somewhat abundant in the south this year; also a specimen of the striped hawk moth, *D. livornica*, caught at Brighton this year, and varieties of the poplar hawk moth, *S. populi.*

Mr. Sewell exhibited a bone, obtained at a depth of 14 feet in Norfolk Square, which resembled a human *tibia* more than the bone of any of the animals,—such as the horse, ox, elephant, or deer,—associated with the Post-Pliocene.

Mr. C. Smith exhibited and presented for the Society's Herbarium two mosses, new to Britain, recently discovered by Mr. Mitten in Sussex, *viz. Pottia littoralis*, found near Aldrington, and *Pottia asperulosa*, near Hastings. They had not yet been described, nor had they been seen by the Botanical world at present. He had been fortunate enough to find one of them since Mr. Mitten's discovery.

December 22nd.—Microscopical Meeting. Mr. Glaisyer, Vice-President, in the chair.

Mr. R. Glaisyer announced the receipt of twelve slides from Mr. Gwatkin, three from Mr. Wonfor, and two slides of spicula of the new sponge *Pheronema Grayi*, obtained in the "Norna Expedition," off the coast of Spain, from Mr. Marshall Hall, the owner of the yacht; and a reprint from the 'Monthly Microscopical Journal' of a paper on the same sponge by Mr. W. S. Kent. Votes of thanks were given to these gentlemen.

Mr. Hennah called attention to a series of cheap lenses by Gundlach, very kindly lent to him by Mr. T. Curties, of the establishment of C. Baker, 244, High Holborn, the agent for England, for exhibition before the Society. Since the last meeting he had had an opportunity of carefully examining, through the courtesy of Mr. Curties, a series of Gundlach's lenses, ranging from a $\frac{1}{2}$ to a $\frac{1}{24}$th. The $\frac{1}{2}$ and $\frac{1}{4}$th were not good, the $\frac{1}{3}$rd was a perfect lens, the same might be said of the $\frac{1}{12}$th and $\frac{1}{16}$th. The last, which was on the immersion principle, was especially useful, as giving 600 diameters with a low eye-piece, and working through thick glass. No recent additions to the microscope have equalled these lenses as instruments of research in minute natural history. The $\frac{1}{12}$th, though very good, yet being a *dry* lens, approached nearer than the $\frac{1}{16}$th, and was in power equivalent to a Ross's $\frac{1}{8}$th; they also worked through thick glass, and were not inferior to any lenses he had seen, though Ross's $\frac{1}{8}$th was superior on *P. angulatum.* They would be very valuable to those who did not possess English lenses of this power, especially as in the whole series the price of the objectives was about *one-third* that of English ones. Finding the first specimens so good, he had asked Mr. Curties to send others, so that they might be able to judge if the supply would be equal to the specimens first sent. As far as he had been able to judge, they were still of the first class. He felt personally indebted to Mr. Curties for his kindness in allowing him time for a fair judgment, and thought the members, as microscopists, were under an obligation to him for the opportunity he had afforded them.

Mr. Sewell, who had a $\frac{1}{3}$rd out of the first batch, was well satisfied with its performance, and could not wish for a better lens.

Mr. Wonfor expressed himself as well pleased with what he had already seen, and doubted not the members would be delighted with these lenses, not only as regarded their performance, but also their cheapness. He would suggest that the objects to be shown that evening should be seen with Gundlach's lenses, and a comparison be made with other objectives by the members present. Upon all occasions Mr. Curties had acted in a most courteous and kindly way to the Society, and he should have great pleasure in proposing a cordial vote of thanks to him for this and other acts of kindness.

Mr. Sewell seconded the resolution, which was carried unanimously.

Mr. Wonfor then introduced the subject for the evening, "Shell Structure," by describing the structure and component parts of the shells among the Mollusca, at one time supposed to be mere inorganic exudations, cemented together by animal glue. It was now known that their shells were composed of animal and calcareous matter, the first constituting a membranaceous basis, forming cells, with hexagonal walls or laminæ, more or less wrinkled; some shells were traversed by tubes, others by canals, with trumpet-shaped orifices; in each case the calcareous matter giving solidity to the membranaceous tissue. The internal layer, of a nacreous nature, was in many very beautiful. Some of the porcellaneous shells were made up of three distinct layers of a similar structure. In the Crustacea, especially the crab, four layers could be distinctly made out, one of which strongly resembled *dentine*, except that the *tubuli* did not branch, but remained of the same size throughout. It was in the cellular layer where the pigmentary matter, which imparted the colour to the shell, was to be found. The membranaceous matter could best be made out by decalcifying the shell and cutting thin sections.

The meeting then became a conversazione, when Mr. Hennah exhibited, under Gundlach's $\frac{1}{12}$th and $\frac{1}{16}$th, sections of crab and cowrie-shell, mounted with thick covers. The performance was perfect. Afterwards, for lined objects, *Pleurosigma angulatum* and tasselled scales of white butterfly were shown. The $\frac{1}{16}$th gave very good results; but the $\frac{1}{12}$th did not equal Beck's popular $\frac{1}{8}$th.

Mr. Sewell exhibited with a $\frac{1}{3}$rd, sections of pearl and crab-shell. This lens was pronounced very good.

Mr. Peto, under a $\frac{1}{2}$ and a 1 inch, with convex front, exhibited sections of pinna-shell. Both lenses were excellent.

Mr. Wonfor, under a couple of $\frac{1}{3}$rds, exhibited egg-shell of garden snail, sections of shell of terebratula, shells of prawn, shrimps, &c. These lenses were very good, and when compared with Mr. Sewell's $\frac{1}{3}$rd to test equality of performance, were so much alike that scarcely a difference, if any, could be detected.

Mr. R. Glaisyer exhibited shells of nautilus, &c., cut through to show their chambered character.

It was resolved that the subject for the January Microscopical Meeting should be "The Use of the Polariscope in the Determination of Structure."

January 12th, 1871. Ordinary Meeting. Mr. F. Merrifield, President, in the chair.—Captain Walker and Mr. W. Saunders were elected ordinary members.

Mr. Wonfor, Hon. Sec., reported the receipt for the library of the 'Third Annual Report and Proceedings of the Eastbourne Natural History Society' from the Secretary, and a pamphlet, by Dr. Stevens, 'On the Flint Implements found at St. Mary Bourne,' from the author. Votes of thanks were passed to the donors.

Mr. J. Howell read a paper "On the Brighton Cliff Formation" as revealed by the Main Drainage excavations.

Reading Microscopical Society.*

January 3rd, 1871.—Captain Lang presided, and Dr. Shettle resumed the reading of his paper, "On the Action of Electricity in the Formation of Nerve Cells and Fibres;" more particularly referring to Lockhart Clarke's investigations as to the structure of the spinal cord in the fœtal sheep, and from them in connection with the laws of electrical action, deducing conclusions as to the formation of nuclei; the aggregation of clusters of nuclei; the aggregation of nuclei and formation of cell walls around them; their conversion into elongated fibres, and the formation of cells of sigmoid shape.

Mr. J. C. Simpson read a paper "On the Electrolytical Coagulation of Milk." After a few words in justification of bringing forward (what might be considered) a physical phenomenon in connection with a professedly Microscopical Society, the writer stated how, upon one occasion, when testing the action of a small voltaic battery and finding its action too strong, he was led, by accident, and as a matter of convenience, to interpose a little slightly-milky water in the circuit, and soon noticed a sort of ring forming round the end of one of the immersed wires.

Having then referred to the microscopic and chemical characters of milk, and to its coagulation by acids, he described the appearances presented by the passage of a current of electricity, as seen under the microscope. A drop of a watery solution of milk was placed on a slip of glass, and under the usual thin glass cover electricity being transmitted, gases were given off abundantly at both poles. Around the negative pole the milk globules were thinned out, and seemed to repel each other, while round the positive pole they seemed to coalesce. At the same time a sort of ring or wave spread out from the positive pole. Then suddenly, and, rather nearer to the positive pole, a brownish line or fold appeared; the globules quickly flowed up to it on both sides, and it gradually lengthened and widened, until it formed an irregular curve round the positive pole. This flow of globules soon ceased upon the positive side, but continued on the negative side. The centre of this wall of milk globules became more and more transparent, while the wall itself grew thicker. On reversing the current, the transparency at once disappeared, and a line of globules started off from both sides into the liquid, but on again transmitting the current as before they were again attracted. On permanently changing the direction of the current this wall was entirely dispersed and reformed near the opposite pole. The wall itself was seen as a thin transparent gelatinous tissue rising from the surface of the slide, and was considered to be a true coagulation.

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* Report supplied by Mr. B. J. Austin.

A precisely similar experiment upon a drop of water containing a few grains of fine flour, showed the formation of a similar wall; but in this case the wall quickly resolved itself into a spiral-shaped cloud of very fine particles, which spread out indefinitely towards the positive pole; and soon after vortices were seen in this mass of particles, while within them plates and rings of the particles themselves were seen revolving with different degrees of rapidity. The motion stopped at every break of the current.

That the flow of milk globules was not due to the flow of gaseous bubbles (evolved by the electrolysis) was proved by repeating the experiments under conditions which allowed the gas to rise at once into the air; and also under conditions whereby no gas was evolved, when a similar flow of particles in the same direction uniformly occurred.

The writer's explanation was that the phenomenon arises from the electrolysis of the water in the milk, and that the coagulation takes place in the casein, but he believes that the experiments of Jürgensen and of Quincke upon the mechanical transport of fine particles from one pole to another, do not account for the somewhat similar motions observed by him in the coagulation of casein and other albuminoids.

The same methods of investigation being applied to albumen were found to lead to different results, according to the strength of the solution; undiluted egg-albumen at once yielding a thick wall round the positive pole. From a thinner solution the wall is strangely broken and renewed, while from one thinner still the coagulum forms between the two poles, but is soon broken up into dust-like particles.

The fact that albumen coagulates at the positive pole is well known, having been observed by M. Lasaigne; and the conversion of albumen into fibrin has been described by Mr. A. H. Smee, in a paper published in the 'Proceedings of the Royal Society' for 1863.

A watery solution of blood serum yielded a coagulum at the junction of the waves from the poles, the side towards the negative pole being of a bright scarlet colour, while that towards the positive pole appeared green. In the case of undiluted fresh blood, coagulation began near the positive pole, but there was no attraction of disks.

A solution of legumin from bitter almonds presented similar appearances to milk, except that the coagulum was darker and more tenacious, but the bright median portion was wanting.

February 7th, 1871.—Dr. Shettle concluded the reading of his paper "On the Evidence to be obtained as to the Nature of the Vital Force, from a minute study of Anatomy, and of the Laws which regulate the Electro-Magnetic Force." This last portion of the paper more particularly referred to the action of electric currents in the production of cell-walls and cells which, becoming polarized, would elongate or become crescentic, and by an extension of their lines of force would even form tubes, as in nerve-tissue. The rotation of the frog-embryo within the egg was considered due to dextrorsal currents. The source of this electricity was referred to a magnetic condition of the arterial blood from its absorption of oxygen; and experiments as to the magnetism of arterial and venous bloods were cited in support of this idea. The bi-concave form of the blood corpuscles, their adherence by their flattened surfaces, the connection of osmosis with electricity, and finally Dr. W. B. Richardson's experiments as to the conducting power of blood, were held to establish the theory.

Captain Lang exhibited the eggs of a parasite (order *Anopleura*, genus *Docophorus*) found in vast numbers on the down of a kestrel hawk lately dead. Examination after death showed that almost all the down feathers had been eaten away, whilst on the few remaining feathers these eggs were most abundant. They were mere empty shells, yet but *one* dead parasite and no living ones could be seen. It was presumed that the death of the bird (otherwise apparently healthy) resulted from exposure to cold in consequence of the loss of its downy covering.

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Biologische Studien. Studien über Moneren und andere Protisten nebst einer Rede über Entwickelungsgang und Aufgabe der Zoologie. Von E. Haeckel. Leipzig. Engelmann.

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Histologie générale. Étude critique sur Virchow et la Pathologie cellulaire. Par P. Jousset. Paris.

Beiträge zur Histologie des Gehörorganes. C. Rudinger. Munchen.

V.—*On the History, Refractions, Definition, and Powers of Immersion Lenses and New Refractometers.*

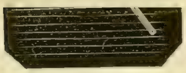
By ROYSTON-PIGOTT, M.A., M.D. Cantab., M.R.C.P., Fellow of the Cambridge Philosophical and the Royal Astronomical and Microscopical Societies of London, formerly Fellow of St. Peter's College, Cambridge.

(*Part I.*)

I.—HISTORY has handed down to us the labours of CLAUDIUS PTOLEMY, the Father of Optics. His celebrated experiment of rendering a coin visible placed in an empty vessel by filling it with

NOTICE TO BINDER.

In consequence of an error in page 66, Number XXVI., a corrected leaf is now sent for insertion.



Bent Oar in Water.

Refraction from Air into Water.

His very ingenious mode of settling these points, as it really involves the important principle of the MODERN IMMERSION LENS, may not be uninteresting to those unacquainted with the experiments. Observing the crooked image of an oar in water, and the curious apparent rising of the money as the water is poured into the vessel, he had a semi-cylinder of "pure glass" constructed, to which he adjusted a graduated circle, so that the diameter of the cylinder coincided precisely with the diameter of the graduated circle above mentioned, and also with the surface of the water. A small coloured body was placed at the centre of the circle; a second similarly fitted to one of the quadrants out of the water; a third slided on the lower part of the graduated circle immersed in the water. Observations were then made upon the angles of incidence and refraction of such a degree of accuracy as to excite our admiration after a lapse of nearly two thousand years. The determination of the conditions of refraction through A WATER LENS was thus anciently effected sufficiently exact for all the practical purposes of modern theory.

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regulate the Electro-Magnetic Force." This last portion of the paper more particularly referred to the action of electric currents in the production of cell-walls and cells which, becoming polarized, would elongate or become crescentic, and by an extension of their lines of force would even form tubes, as in nerve-tissue. The rotation of the frog-embryo within the egg was considered due to dextrorsal currents. The source of this electricity was referred to a magnetic condition of the arterial blood from its absorption of oxygen; and experiments as to the magnetism of arterial and venous bloods were cited in support of this idea. The bi-concave form of the blood corpuscles, their adherence by their flattened surfaces, the connection of osmosis with electricity, and finally Dr. W. B. Richardson's experiments as to the conducting power of blood, were held to establish the

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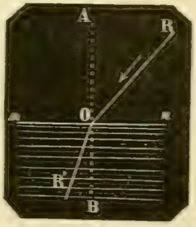
V.—*On the History, Refractions, Definition, and Powers of Immersion Lenses and New Refractometers.*

By Royston-Pigott, M.A., M.D. Cantab., M.R.C.P., Fellow of the Cambridge Philosophical and the Royal Astronomical and Microscopical Societies of London, formerly Fellow of St. Peter's College, Cambridge.

(*Part I.*)

I.—History has handed down to us the labours of Claudius Ptolemy, the Father of Optics. His celebrated experiment of rendering a coin visible placed in an empty vessel by filling it with water, was perhaps the first scientific attempt at experimental refraction: thence he proceeded to measure the refraction of light passing from water into air, from air into glass, and lastly from water into glass. He sagaciously avoided the deviation from glass into air by taking care the rays should emerge perpendicularly to the surface of the glass.

Bent Oar in Water.



Refraction from Air into Water.

His very ingenious mode of settling these points, as it really involves the important principle of the modern immersion lens, may not be uninteresting to those unacquainted with the experiments. Observing the crooked image of an oar in water, and the curious apparent rising of the money as the water is poured into the vessel, he had a semi-cylinder of "pure glass" constructed, to which he adjusted a graduated circle, so that the diameter of the cylinder coincided precisely with the diameter of the graduated circle above mentioned, and also with the surface of the water. A small coloured body was placed at the centre of the circle; a second similarly fitted to one of the quadrants out of the water; a third slided on the lower part of the graduated circle immersed in the water. Observations were then made upon the angles of incidence and refraction of such a degree of accuracy as to excite our admiration after a lapse of nearly two thousand years. The determination of the conditions of refraction through a water lens was thus anciently effected sufficiently exact for all the practical purposes of modern theory.

Ptolemy observed that the refractions from water into glass were less than any he had previously observed. The following were some of the results he obtained:—

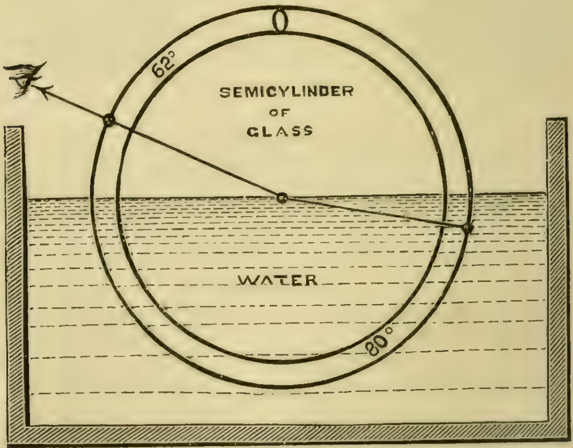
Claudius Ptolemy's Tables.

Refractions from Glass into Air.			Refractions from Water into Glass.		
Angle of Incidence.	Angles of Refraction.		Angle of Incidence.	Angles of Refraction.	
°	°	′	°	°	′
0	0	0	0	0	0
20	13	30	20	18	30
60	34	30	60	49	30
80	42	0	80	62	0

The first Table on the left, for the 4 angles, gives the value of the refractive index for Ptolemy's "pure glass" at 1·48, 1·53, 1·47; the mean of which is 1·49, or nearly the refractive index for our common glass, and evidences the singular care Ptolemy must have taken in his experiments.

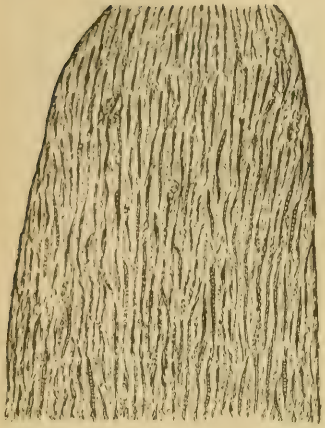
For the water refractions the values of the index of refraction between glass and water are (by the refractometer, to save the calculation by logarithms), for 60° and 80° in the Table, nearly 1·500 and 1·336, a surprising approximation to the true values by this primitive method.

Ptolemy's Immersion Lens.

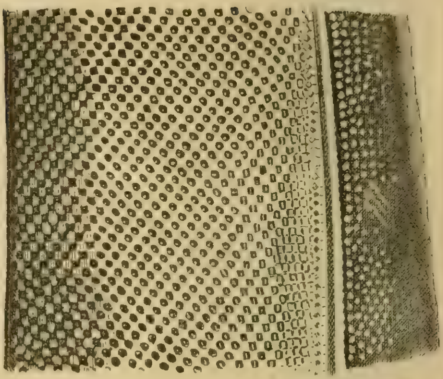


Refraction from Water into Glass.

If the glass cylinder be removed, then the ray would be turned back by the surface of the water, and internally reflected, instead of being refracted.

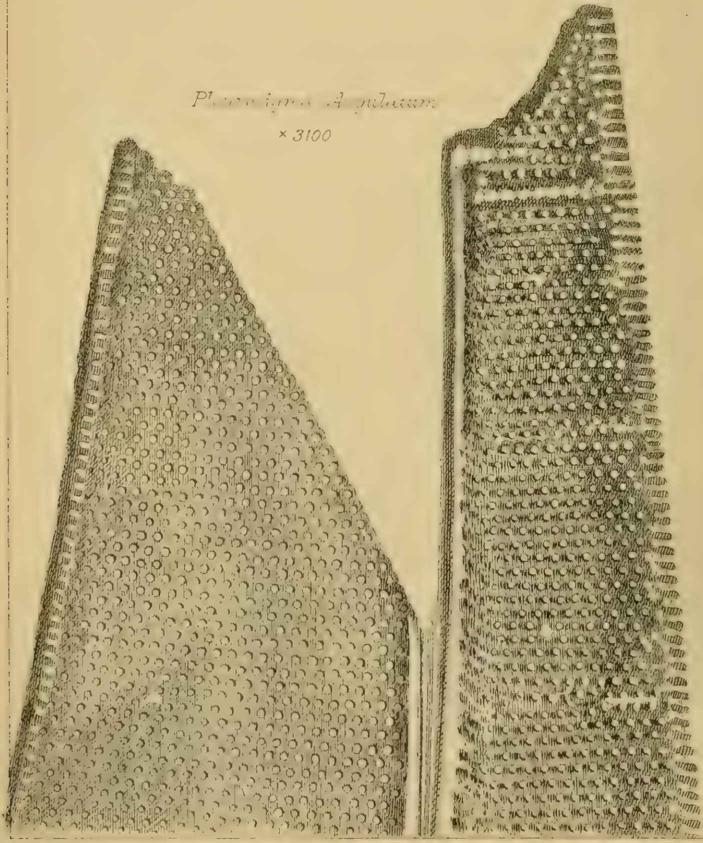


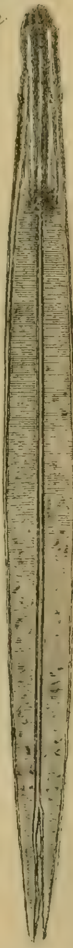
Scale of Degeeria Nigromaculata
× $\frac{2300}{2}$



Portion of Pleurosigma Formosum
× $\frac{3100}{2}$

Pleurosigma Angulatum
× 3100





Amphipleura Pelucida × 960.

W. West & Co imp

Copies (reduced) of Dr Woodward's Photographs

THE

MONTHLY MICROSCOPICAL JOURNAL.

APRIL 1, 1871.

I.—*On the Structure of the Podura Scale and certain other Test-objects, and of their Representation by Photo-micrography.*

By Lieut.-Col. Dr. J. J. Woodward, U. S. Army.

(*Read before the* Royal Microscopical Society, *March* 1, 1871.)

Plates LXXIX. and LXXX.

The 'Monthly Microscopical Journal' for December, 1869, contains a paper "On High-power Definition," by Dr. G. W. Royston-Pigott. This essay was rapidly followed by others on allied subjects, the whole constituting a remarkable series. They contain mathematical discussions of many points connected with the optical questions involved in the use of objectives, and especially of immersion objectives; assert the existence of "a certain residuary aberration, chiefly spherical," in the best modern objectives;[*] claim the correction of this aberration, partly by special modes of illumination, but chiefly by means of a vaguely-described optical combination, "the Aplanatic Searcher;"[†] and finally, contain many interesting and some novel statements with regard to the structure of several test-objects, and especially the Podura scale.

These papers have provoked considerable discussion in England, and perhaps a few temperate remarks on the subject by a Transatlantic observer will not be without interest.

My observations will relate chiefly to the structure of the Podura scale and a few other test-objects, and to the possibility of representing these objects photographically.

The mathematical portions of Dr. Pigott's papers must be left to such appreciation as the mathematicians may award; but his general assertion that our finest objectives are not perfectly corrected, is doubtless substantially true. It is well known that the lenses sent out by the best makers continue annually to improve; and I, for one, should be sorry to believe that we have yet reached the limit of perfectibility. How far the "Aplanatic Searcher" can be regarded as a valuable addition to the optical appliances at the disposal of the microscopist, is a question on which, perhaps, any judgment of mine would be premature. Last July I wrote to Messrs. Powell and Lealand, requesting them to make one for the Army Medical Museum, intending to try it in connection with the immersion $\frac{1}{16}$th of these makers. They were at the time, as I understood, constructing one for Dr. Maddox, and I asked them, so soon as that was finished to the satisfaction of Dr. Pigott, to make the one for the Museum. This instrument has not yet reached me, and from the information afforded by my London correspondent, I fear I shall not receive it at any very early day. I am, however, not unwilling to admit that demonstration alone could lead me to believe that *any* optical combination introduced into the body of the microscope, between the eye-piece and the objective, would result otherwise than in loss of light and impaired definition. Especially should I anticipate this result, if the optical combination was such as to increase the magnifying power. This is the result with all the various achromatic amplifiers I have experimented with, and I have learned nothing as yet of the "Aplanatic Searcher" which would lead me to suppose it escapes from the same defects. I would suggest to Dr. Pigott a simple practical test that would go far towards dispelling the doubts on this subject, which, so far as I have been able to learn, are very generally entertained. This test is afforded by an examination of Nobert's plate with and without the Searcher. Can an objective which with eye-pieces alone just resolves any given band on the test-plate be made to resolve the next higher band by the Searcher? Will not its performance rather fall to the next band below, or even farther? It would also be interesting to know whether the nineteenth band of Nobert's plate can be resolved by *any* objective used with the "Aplanatic Searcher."

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[*] 'Monthly Microscopical Journal,' vol. ii., p. 296.

[†] Ib., vol. iv., p. 42.

With regard to the Podura scale, it gives me great pleasure to express the opinion that Dr. Pigott's observations constitute a valuable addition to our knowledge of this test. On the coarser Degeeria scale I have had no difficulty in making out appearances which, so far as I can gather from Dr. Pigott's own descriptions and the published discussions with regard to his views, are substantially the same as seen and shown by him. And even on the more minutely marked and difficult Lepidocyrtus scale I have been able to develop images which seem to be substantially similar. In arriving at these results I have used chiefly the immersion $\frac{1}{8}$th and $\frac{1}{16}$th of Powell and Lealand, with the ordinary eye-pieces.

When Dr. Pigott's paper in the 'Monthly Microscopical Journal' of December, 1869, first reached me, my attention was especially drawn to his novel views with regard to the Podura scale by the personal allusion to myself on page 299. The paragraph is rather indefinite, apparently owing to a misprint; but I understand the language used as intimating that it was chiefly the examination of certain photographs of the Podura scale, made at the Army Medical Museum, which induced him to publish at that time the results of his previous, but still incomplete, studies. As these photographs were all made with dry lenses, I took an early opportunity to re-examine the scale with the immersion $\frac{1}{16}$th of Powell and Lealand. But although direct sunlight reflected from a plain mirror gave glimpses of a structure distantly resembling the new descriptions, the resolution was so unsatisfactory that I was inclined to regard the appearances as spurious, and more convinced than ever that the scale was properly resolved when the exclamation marks were shown. I made the two photographs sent with my note of April 23, 1870, to Mr. Hogg.* Copies of these photographs accompany the present paper. No. I. shows the notes of exclamation as figured by Mr. Richard Beck, in his work on the Microscope. In No. II. there is a pretty distinct knob at the head of each note of exclamation. Both photographs are magnified 3000 diameters by Powell and Lealand's immersion $\frac{1}{16}$th.

In my note to Mr. Hogg I expressed two wishes, both of which, I am happy to state, have been gratified. The first was, that he would obtain for me, from Dr. Pigott, one or two of the slides used for his observations; the second, that my friend Dr. Maddox might be induced to photograph the Podura scale as seen by Dr. Pigott.

In response to the first of these wishes, I received from Mr. Hogg two slides; one of the test Podura, the other labelled Degeeria Nigro-maculata;† the first from Dr. Pigott, the second from Mr. S. J. McIntire. To these gentlemen I desire to express my sincere thanks.

The response to my second wish was not less satisfactory. Dr. Maddox undertook the photographic representation of both Degeeria and test Podura, and has kindly sent me copies of the interesting series of pictures exhibited by him to the Society last June.‡

Under these circumstances I renewed my observations, with the intention of making a series of photo-micrographs representing such appearances as I should be able to find.

And here I desire to offer a few remarks with regard to the real significance of photo-micrography, as a means of recording honestly, and without modifications of subjective origin, the appearances actually seen in the case of difficult test-objects.

In his paper "On the Diatom Prism"§ the late lamented President of the Royal Microscopical Society, the Rev. J. B. Reade, makes a remark which Dr. Pigott interprets as underrating this mode of representation. After describing the arrangement of the hemispherical elevation on *Pleurosigma quadratum* and *P. angulatum*, he says—"Hence, under the illusion of the common methods of illumination, which deal with shadows only, and under deep powers, the markings of those diatoms are described and figured as hexagons, with the sides and centres light and dark, or *vice versâ*, and PHOTOGRAPHY stands by as an attesting witness."

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* 'Monthly Microscopical Journal,' vol. iii., p. 324.

† Sir John Lubbock, in his "Notes on the Thysanura," 'Lin. Soc. Trans.,' vol. xxvii., p. 277, names this scaled species of Degeeria, *Seira domestica* (*seira*, a chain), from the circumstances that it "forms a very interesting link (or chain) between several genera, some of which approach very closely to Lepidocyrtus," &c.

‡ 'Monthly Microscopical Journal,' vol. iv., pp. 51, 67. § Ib., vol. ii., p. 5.

Dr. Pigott, in his paper published last November,* quotes this sentence and expresses himself still more strongly. Speaking of the beading he describes on the Podura scale, he says:—"It would seem the actinic glasses fail at present to perfect these views. Illumination is a vexed question for the photographer, who has never yet succeeded in displaying the spherules of the *Amphipleura pellucida*, which so many English observers have seen microscopically. Unless, therefore, the defining power of photography at least equals the human eye armed with the microscope, no reliable argument can be drawn from its failure or approximate revelations. Indeed, the President notices that photography indicated hexagon forms as the correct appearance for the diatom beadings, which we now know is false."

I do not think the President's remarks were intended to express the notion they are here quoted to support, and I believe I shall be able to show that the paragraph from Dr. Pigott's paper is inaccurate in every particular. Indeed I am at a loss to understand how some of the misconceptions it contains could have arisen at this late day.

In my first essay on photo-micrography, published in the 'American Journal of Science and Arts,' vol. xlii., September, 1866, I expressed the opinion that photography was "adequate to the satisfactory representation of all microscopical objects that do not depend for their value on colours." Subsequent experience has satisfied me of the substantial justice of this opinion.

In examining objects with the microscope with white light, both colour and form produce impressions upon the eye, and of course it must be freely granted that photography cannot be counted upon as a means of representing the colour phenomena. If, however, the microscope be illuminated by monochromatic light the colour disappears and form alone is perceived, the object appearing black or shaded on a ground of the colour employed. A trial will convince anyone that so far from impairing definition, this method considerably improves it, and for mere purposes of observation it matters little what kind of monochromatic light is employed. I find blue or violet light most agreeable to the eye, and these are also the colours suitable for photography; but for mere observation any other colour will answer. Such light may be obtained from the

* 'Monthly Microscopical Journal,' vol. iv., p. 254.

solar rays or from the electric lamp, either by means of a prism as recommended by Count Castracane, or still more conveniently by passing the rays through a coloured solution. I use, ordinarily, a solution of the ammonio-sulphate of copper, which gives a bluish-violet light approximating monochromatism sufficiently for all practical demands, whether of observation or photographic representation. When I compare the images of a difficult test as seen with any objective by such monochromatic light, with the best attainable with the lamp-light illumination ordinarily employed, I must confess to a feeling of astonishment that this simple and potent optical resource is not more generally employed by microscopists, aside from all questions of photography. Not the least of its advantages is the fact that in the case of those objects which decompose light readily, the optical effects produced by this circumstance are at once eliminated, and the observer is consequently better able to judge reasonably with regard to the actual form.

Now, in the case of transparent objects, I do not hesitate to assert that whatever can be discerned by "the human eye armed with the microscope" if monochromatic illumination is employed, can be copied photographically in a manner that faithfully represents every detail seen. And such representations have this superiority over mere drawings or descriptions, that they are purely objective, and the imagination or enthusiasm of the observer, which plays so considerable a *rôle* in most drawings and descriptions, is wholly excluded.

I cannot, therefore, admit Dr. Pigott's statement that illumination is a "vexed question for the photographer," and his allusion to the failures of "the actinic glasses" is apparently penned without a consideration of the fact, that the immersion objectives of high powers, which he so justly praises, require no special corrections to fit them for photographic use, and are the very ones actually employed. As for the *Amphipleura pellucida*, there is nothing to prevent anyone from photographing it quite as well as he may be able to see it; to this, however, I shall recur again.

I sincerely believe that if microscopists generally would take the trouble to master the simple steps necessary to enable them to record their actual observations by photography, many bitter and unnecessary disputes would be avoided, and the solid progress of inquiry into minute structure would be materially advanced. I am not, however, disposed to exaggerate the just importance to be attached to photographic representations, and must therefore add a few words with regard to the real limitations of their usefulness.

All that can properly be claimed for photo-micrographs is, that if the chemical work is well done, the picture will be a faithful representation of whatever the observer is able to discern with monochromatic light. Success in the chemical process is best and most economically secured by employing a professional dark-room man, and any competent observer can judge of the quality of this portion of the work, by examining the pictures themselves. The microscopist who directs the work is best employed in seeing the object, which, if he follows the simple methods I have described in previous papers, will secure an image, of all he sees, on the sensitive plate; but nothing more. There is no charm in photography which can make it a touchstone of truth. If the objectives are imperfect; if the illumination is faulty; if the object is dirty or improperly prepared, the photograph will tell the whole story as no drawing or description would do. Others will be in a position to judge the faults of the microscopist's work, and this by the way is no doubt one reason why so many observers shrink from the cruel sincerity of this method of representation. When our improved objectives first showed the markings on the Angulatum as hexagons, photography faithfully reproduced them. When higher powers of better definition showed the hexagons to be imperfectly seen circles, photography had no difficulty in representing these. The hemispheres rendered visible by a still further advance are shown in the photographs which accompany this paper, and any future progress resulting from better optical appliances will be chronicled in the same manner.

It is sometimes thoughtlessly objected that photo-micrography reproduces the phenomena of interference or diffraction, as well as the real structure. This I readily admit, and claim as an advantage, for these phenomena are actually seen by the observer. If he merely describes or draws, he omits all that he supposes belongs to this category, and by so doing omits sometimes the essential; the photograph omits nothing, and anyone who examines it is placed more nearly on the plane of the original observer than he could be by any other means, unless indeed he could sit by the observer's side and look over his shoulder. This first limitation of the interpretation of micro-photographs, therefore, is in fact only a guarantee of their optical honesty.

There are other and different limitations, however, which require mention. Thus our efforts to represent opaque objects, particularly as seen with medium and high powers, have not as yet been crowned with any great success. How far this results from the small amount of labour hitherto expended in this direction, is not to the purpose; the success is not yet attained. So also there are certain yellow objects, such as parts of insects, for example, which transmit so little violet light that as they are ordinarily prepared, photography fails to exhibit all the details discernible by the eye. This difficulty, however, is readily escaped by resorting to suitable bleaching processes in the preparation of the objects, and these very processes greatly improve the character of the objects for observation by the eye.

Let me now return to the question of the structure of the Podura scale. I began my investigation on the slide of *Degeeria Nigro-maculata* sent by Mr. McIntire. The markings on these scales were very bold, and the object was in every way a beautiful one. As I first arranged the illumination the markings resembled the notes of exclamation as seen on the ordinary test Podura, less rounded at the heads, however, less pointed at the opposite extremity of each marking, and the whole marked obliquely across by a delicate series of transverse lines which were observed most distinctly between the notes of exclamation, but which could readily be made to appear to cover these also, when the fine adjustment was toyed with. I soon found, however, that by proper management of the illumination it was easy to make an appearance "start into view" which recalled at once the descriptions of Dr. Pigott, and Fig. 7 of Plate XXXIII. accompanying his paper of December, 1869. In that paper, it will be remembered, he asserted that the notes of exclamation are mere optical effects produced by two sets of beads arranged in "rouleaus" which cross each other at a small angle.

He says, of the beads on the surface of the scale nearest the observer:—"But with 2300 diameters they appear to lie on the same plane and terminate abruptly on the basic membrane: upon focussing for the strings of beads attached to the lower side, the beading appears in the intercostal spaces. The upper beads are best seen either green upon a pink ground or pink upon a greenish ground." And farther on:—"By estimation, comparing these beads with those of the *P. Formosum* of $\frac{1}{30000}$th inch in diameter, the observed Podura beads may be reckoned at $\frac{1}{50000}$th to $\frac{1}{150000}$th of an inch in diameter. The spines usually drawn really embrace in general three or four beads."

I understood, at the time, the foregoing description to be intended for the scales of *Lepidocyrtus curvicollis*, and this opinion is confirmed by the remarks of Dr. Pigott in a subsequent paper;* but from the statement of Mr. McIntire that the Podura scale exhibited by Dr. Pigott to the Microscopical Society was really a scale of Macrotoma,† I presume that the coarser scales aided him in arriving at his interpretation of the structure of Lepidocyrtus. In any event, in his paper published in January, 1870, the same structure is predicated of Macrotoma and *Degeeria domestica*, the scales of which are said to "render that easy which is exceedingly difficult in the regular test scale." We are further told, in the same paper, of the Degeeria,—"A lady who is now viewing the scale says, 'the beads look just like rows of peas in a pod.'"‡

The description given by Dr. Pigott in his first paper, as quoted above, may be taken as an approximate account of what I saw on the *Degeeria Nigro-maculata* of McIntire.

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* 'Monthly Microscopical Journal,' vol. iv., p. 305.

† Ib., vol. v., p. 12.

‡ Ib., vol. iii., p. 13.

The so-called beading was readily brought out with any power above the half-inch. Immersion lenses gave more satisfactory views; and the very best effects being obtained by the immersion $\frac{1}{16}$th of Powell and Lealand, I selected this lens for preparing the photographs. The appearance sought was readily obtained by oblique light thrown lengthwise upon the scale from the concave mirror, whether the illumination was afforded by a coal-oil lamp or by the sun; but the best results were secured by using the achromatic condenser of Powell and Lealand, diaphragmed down so as to transmit a pencil of small aperture, and obtaining obliquity by decentering the condenser somewhat by means of the screw movements of the secondary stage. Many other tricks of illumination were tried, but the above gave, on the whole, results most nearly comparable with Dr. Pigott's description. I found the so-called beading was best shown when the cover correction was so arranged as to show the exclamation marks most distinctly. If, then, the illumination was managed for the best display of the upper beading, I found on many of the scales that a slight depression of the objective, by means of the fine adjustment, brought the lower beading into view, and with a still further slight depression the notes of exclamation were seen. By slowly raising the objective in the same manner, the phenomena were reproduced in reverse order. On some of the scales, however, which I suppose to be those which lie with their under-surface towards the observer, as the objective was lowered the exclamation points were first seen, then the two sets of beading, one after the other. The colours of the beading appeared to me to be alternately greenish and reddish (I should hardly choose the word pink), according to the position of the fine adjustment. When the upper rows of beading appeared reddish, the lower rows were greenish, and *vice versâ*. The beading, however, at its very best, always resembled the "rows of peas *in* a pod." I praise the eyes of the lady. They ran into each other; they appeared, rather, as irregular ribbings, with alternate varicose swellings and constrictions, than as rows of such isolated spherules as are seen on so many diatoms. In size they varied somewhat, but rows of five or six seldom measured less than $\frac{1}{10000}$th, and often as much as $\frac{1}{6000}$th, of an inch long. The colours were most brilliant and the approximation to the spherical form closest when the sunlight was used. The handsomest results were obtained when the direct rays of the sun were allowed to fall upon the mirror at such an angle that just so much light should pass the condenser as the eye could bear without being dazzled. On interpolating in the solar pencil a cell containing a solution of the ammonio-sulphate of copper, the definition was materially improved, but the varicose ribs looked now even less like rows of isolated beads than before. The photographs show the appearances attained in this manner.

Turning now to the Lepidocyrtus scale, I first examined the one kindly sent me by Dr. Pigott. The slide was labelled as prepared by Topping, and marked "1850, slide twenty years old." An accompanying note from Dr. Pigott to Mr. Hogg says:—"The only good scale is at the edge. The finest old-fashioned scales have become exceedingly scarce." Marks on the slide enabled me without difficulty to identify this scale, which was of great size, being nearly the $\frac{1}{100}$th of an inch in length. It was, however, disfigured by two much smaller scales lying transversely beneath it, and by granular *débris* which obscured a portion. Still, enough of the scale was visible to serve for examination, and on this I had no difficulty in observing a train of phenomena very similar to what I saw on the *Degeeria Nigro-maculata.* The beading, however, was considerably smaller, though the scale was larger than any of the Degeeria scales I selected for study. Encouraged by this result, I worked over all the specimens of *Lepidocyrtus curvicollis* in my possession, including some slides recently received from Mr. E. Wheeler. I was able to develop somewhat similar appearances even in the smallest and most difficult scales. Of course, the smaller the scale the less satisfactory the resemblance to rows of beads; but *varicose ribbing,* more or less closely approximating that seen on the Degeeria, could always be coaxed out, and the effects produced in the Degeeria scales by raising and lowering the objective could always be more or less distinctly obtained.

It must also be remarked that alike in the coarsest Degeeria and the finest Lepidocyrtus scale, the beaded or varicose ribbing appeared always to my eye less sharply defined than the exclamation markings. And the photographs likewise possess this quality. The same quality will be observed in Dr. Maddox's photographs, already alluded to. I am not disposed, however, to regard this circumstance as an expression of anything more than the fact, easily recognized by the eye when monochromatic light is employed, that the exclamation marks exhibit blacker shadows, and hence afford images (whether to the eye or sensitive plate) possessed of greater contrast than I have been able to give to the beaded ribbing by any management I have yet tried. I must also freely admit that in the smaller Lepidocyrtus scales the beaded character of the ribbing becomes more and more indistinct to the eye, until perhaps its existence is assumed rather on the basis of the similarity of the appearances to those presented by the coarser scales, when these are imperfectly seen, than as an actual objective fact. I presume Dr. Pigott also has experienced the same trouble, or he would not lament the difficulty of finding the "finest old-fashioned scales."

As to the size of the beading in Lepidocyrtus I must regard

Dr. Pigott's estimate $\frac{1}{50000}$th to $\frac{1}{150000}$th of an inch as inaccurate. I suppose $\frac{1}{50000}$th to $\frac{1}{80000}$th of an inch will come much nearer the actual truth. I have carefully measured small portions of the ribbing and counted the number of bead-like swellings between the limits. As the beads, if they may so be called, are in actual contact, in fact, are continuous with each other, no allowance can be made for imaginary interspaces. Dr. Pigott himself gives the elements for correcting his estimate where he says, "The spines usually drawn" (*i. e.* the exclamation marks) "really embrace in general three or four beads." Now, bearing in mind that these beads are in contact, it is only necessary to measure the length of the exclamation marks on the finest Lepidocyrtus scale and divide the measure by four, to see that such a figure as the $\frac{1}{150000}$th of an inch is altogether out of the question.

What then is the true structure of the Podura scale? I am not disposed to enter into any elaborate discussion on this vexed point. Whatever the ultimate decision shall be, we are all under many obligations to Dr. Pigott for teaching us that with the improved immersion objective a new series of phenomena can be observed which had hitherto escaped attention. The true interpretation of these phenomena lies perhaps still in the future, and probably reasonable results are most likely to be attained by such investigations as have been set on foot by Messrs. McIntire and Maddox.*

My object at the present time is simply to record in a tangible form by photography, the actual phenomena as I am able to observe them, in the hope that if improved objectives or other modifications in the mode of observation should enable others to obtain more satisfactory views, mine will at least answer the purpose of encouraging these more fortunate microscopists to record their results in the same manner, and thus to expose them fairly for the instruction and honest criticism of their fellow-labourers.

Nevertheless, I do not hesitate to say that so far as I have been able to arrive at any judgment I am disposed provisionally to agree in the main with one of the possibilities advanced by Dr. Maddox.† I suppose the scale to be composed of two corrugated membranes, each thrown into wavy longitudinal corrugations or ribs, and these again crossed by transverse markings, which make the longitudinal ribs appear varicose (or beaded). I suppose the varicose ribs of the two opposite membranes which compose the scale to cross each other at a small angle. In the proper position of the objective, the upper series of varicose ribs comes into focus, while the lower ones are imperfectly seen out of focus. By depressing the objective slightly the lower ones come into focus, while the upper ones appear out of focus. By depressing it still further the true ribbings are lost sight of, and the exclamation marks, which I am disposed to think with

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* 'Monthly Microscopical Journal,' vol. v., p. 31.

† Ib., vol. iv., p. 53, Fig. 3.

Dr. Pigott are a spurious appearance, come into view. That this train of phenomena is reversed in certain scales would appear to show a difference between the upper and the lower series of ribs.

I send the following photographs in illustration of the above remarks:—

No. III. Photograph of *Degeeria Nigro-maculata;* 3100 diameters.—The scale was ·0035 inch long and ·0018 broad.

No. IV. Photograph of the same scale; 2300 diameters.

No. V. Photograph of another scale of *Degeeria Nigro-maculata;* 2700 diameters.—The scale was ·0034 inch long and ·0013 broad.

No. VI. Photograph of the same; 2300 diameters.

No. VII. Photograph of the same; 960 diameters.

No. VIII. Photograph of the same scale, with several others in view; 960 diameters.

No. IX. Photograph of part of the large scale of *Lepidocyrtus curvicollis* on the slide sent by Dr. Pigott; 960 diameters.

No. X. Photograph of Lepidocyrtus scale $\frac{1}{200}$th of an inch long.

To the foregoing photographs of Degeeria and Lepidocyrtus I add a few of certain diatoms. Long since Mr. Wenham arrived at the conclusion that the markings on diatoms were in fact due to the presence of hemispherical elevations or bosses. Fresh attention was drawn to this view by the paper of the Rev. J. B. Reade "On the Diatom Prism and the true form of Diatom Markings."* Mr. Reade appears to have attributed to his Diatom prism (which I have carefully tried), just as Dr. Pigott has to his "Aplanatic Searcher," the superior definition, which is, in reality, ascribable to the improved quality of the objectives, and especially of the immersion objectives recently produced by certain makers. In point of fact, with modern immersion objectives, and especially with the immersion $\frac{1}{8}$th and $\frac{1}{16}$th of Powell and Lealand, I find myself able to produce better effects with the help of an achromatic condenser, properly managed, than by the Diatom prism.

More important even than the paper of Mr. Reade, however, were the experiments of Schultze on artificial diatoms, to which Mr. H. J. Slack has recently drawn attention in his paper "On the Patterns of Artificial Diatoms."† I have never experimented with these, but have in my possession a number of photographs of them, presented by my friend Dr. Maddox, which are of the most instructive character. That this view of the structure of diatoms is substantially correct I do not now doubt. The details in the case of each individual diatom will, however, require careful study.

No. XI. Photograph of *Pleurosigma Formosum;* 4500 diameters.

No. XII. Photograph of the same; 3100 diameters.—The beading of this diatom is well described by Mr. Reade (*loc. cit.*). I have never been able to see the six black dots surrounding each bead, mentioned by Dr. Pigott.

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* 'Monthly Microscopical Journal,' vol. ii., p. 4.

† Ib., vol. iv., p. 181.

No. XIII. Photograph of Coscinodiscus; 1100 diameters.—This photograph shows the spherical bosses well. It is apparently an undeveloped frustule. On one side the hemispheres are quite a little distance apart. On the other side they are crowded together, producing the spurious appearance of a hexagonal framework with little spherical beads at the corners.

No. XIV. Photograph of *Pleurosigma angulatum*, showing beads; 3100 diameters.

No. XV. The same; 4500 diameters.—In these pictures the illumination has been so managed as to give a considerable degree of perspective to the markings. For comparison, I append a print showing the appearances produced by more central illumination, *viz.*:—

No. XVI. Photograph of *Pleurosigma angulatum;* 3000 diameters.

I conclude this paper with some brief remarks on two difficult diatoms which have been the subject of much recent discussion, *viz.* the *Surirella gemma* and the *Amphipleura pellucida.*

In the case of the *Surirella gemma*, Hartnack, whose original description I have not yet seen, observed fine longitudinal striæ in addition to the fine transverse ones, previously known to exist between the large transverse ribs. He supposed the true markings to have the form of elongated hexagons. These are figured by Dr. Carpenter[*] and by Frey.[†] The mean length of this diatom is given in the 'Micrographic Dictionary' (second edition) as $\frac{1}{240}$th of an inch. A careful examination of specimens mounted dry, has satisfied me that Hartnack's interpretation is erroneous. The fine striæ are, I think, rows of minute hemispherical beads, and the appearance of hexagons is the optical result of imperfect definition or of unsuitable illumination. For photographing this object, I have selected a frustule of somewhat less than the medium size. It measures $\frac{1}{290}$th of an inch in length. Longitudinally, the fine striæ count at the rate of 72,000 to the inch. These striæ are resolved into beaded appearances which count laterally 84,000 to the inch.

No. XVII. Photograph of *Surirella gemma;* 3100 diameters. (Portions of two frustules.)

No. XVIII. Photograph of the same; 1034 diameters. (Two frustules and portions of a third.)

Finally, with regard to the *Amphipleura pellucida* or *Navicula acus.* The striæ on this diatom were first described by Messrs.

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[*] 'The Microscope and its Revelations,' 4th edition, p. 182.

[†] 'Das Mikroskop,' 3rd edition, p. 40.

Harrison and Sollitt, and were estimated by them at from 120,000 to 130,000 to the inch. These measurements have been adopted in the 'Micrographic Dictionary,' and by most of the English writers. Dr. Carpenter and Dr. Pigott are exceptions. Dr. Carpenter has "not been able to satisfy himself, however, of the correctness of Mr. Sollitt's estimate of the distance of the striæ, and is still disposed to regard it as too high."* Dr. Pigott on the other hand makes them as fine as 150,000 to the inch.

Of the American observers, Messrs. Sullivant and Wormley failed to make out the striæ, but this was ten years ago.†

A few months since I received from Messrs. Powell and Lealand a special stage with a small hemispherical condenser, contrived for the purpose of showing the lines on this test. I ultimately found, however, that a pencil of monochromatic light converged by an achromatic condenser of high angle gave better results, provided the condenser was thrown somewhat out of the optical axis of the instrument so as to secure sufficient obliquity. With such illumination, the immersion $\frac{1}{16}$th of Powell and Lealand resolved the smallest frustules; the immersion $\frac{1}{8}$th of the same makers resolved the coarser ones, but was not adequate to resolve the finer.

The best mounted specimens at my disposal were furnished by Messrs. Powell and Lealand a few months since. They wrote me they were not able to obtain cleaner samples at the time. The frustules are dry and are adherent to the lower surface of the thin glass cover, which was disfigured by fine *débris*, as is only too plainly shown in the photographs, though these were taken from the cleanest portion of the slides. The photographs show the transverse markings on the frustules as fine, somewhat beaded, striæ. The analogy of other diatoms justifies the presumption that Dr. Pigott is right in speaking of these striæ as rows of beads. I was not able, however, to make them out as separate beads on the specimens at my disposal. Whenever I can procure some clean frustules, mounted on a thin cover and *with thin underglass*, I will try what I can do further. Meantime I should be glad to learn how nearly the appearances I have photographed correspond with what is actually seen by those English observers who are most familiar with this test.

On one point, however, I have no doubt, namely, the distance of the beaded striæ from centre to centre. As Dr. Carpenter correctly thinks, this has been set down at too small a figure. In fact, there exists in many quarters a disposition to underrate the actual size of many minute objects, which is to my mind a convincing proof of the very imperfect manner in which these must have been seen. For measurement, with at least approximate accuracy, offers no practical difficulties in the case of those objects which can be distinctly observed. Dr. Pigott seems to have fallen into this error with regard to other objects than *Amphipleura pellucida.* Thus for example, he says:*—"It will be extremely interesting to hear that our American brothers can resolve the striæ of known diatoms, having them arranged from 120,000 to 150,000 lines to the inch." This is a rather sweeping statement, and in the absence of a list of the "known diatoms" whose striæ are estimated at this figure, I will confine my remarks to the *Amphipleura pellucida.* Prichard states the length of this diatom at from $\frac{1}{140}$th to $\frac{1}{300}$th of an inch. The 'Micrographic Dictionary' gives the mean length at ·0044. The frustules on the two slides sent me by Powell and Lealand measured from $\frac{1}{170}$th to $\frac{1}{400}$th of an inch.

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* 'The Microscope and its Revelations,' 4th edition, p. 182, *note*.

† 'American Journal of Science and Arts,' vol. xxxi., p. 12, 1861.

From a number of negatives, I send prints of two.

No. XIX. Photograph of *Amphipleura pellucida;* 960 diameters. Frustule $\frac{1}{200}$th of an inch long.

No. XX. Another view of the same frustule; same power.

After making the photographs, I carefully counted the striæ, both on the glass negatives and in the microscope. The frustule selected for the above photographs counted 91 striæ to the thousandth of an inch. Larger frustules exhibited rather coarser, smaller ones rather finer striæ. On the smallest frustules at my disposal, several of them only $\frac{1}{400}$th of an inch in length, I found no example in which the number of striæ exceeded 100 to the thousandth of an inch. The striæ of these smallest frustules do not then rival in fineness the nineteenth band of the Nobert's plate, the study of which I am convinced will make those microscopists who are able to resolve it more careful in their "estimates," with regard to the striæ of diatoms.

In conclusion, I have to add, simply, that all the photographs forwarded with this paper were taken by the immersion $\frac{1}{16}$th of Powell and Lealand, with the exception of the Coscinodiscus, for which I employed the immersion $\frac{1}{8}$th of the same makers.

Note.—Since writing the foregoing my attention has been directed to the letter of Mr. Lobb,† from which it would appear that Mr. Lealand, of the firm of Powell and Lealand, "has succeeded in counting the Amphipleura lines, and finds them 100 in $\frac{1}{1000}$th of an inch." This agrees perfectly with my own results on the smallest frustules. I am happy to find that my testimony against the extravagant "estimates" which have been published, is corroborated by such a witness.

[Plates LXXIX. and LXXX. contain selections from the photographs sent to the Society by Dr. Woodward.—Ed. M. M. J.]

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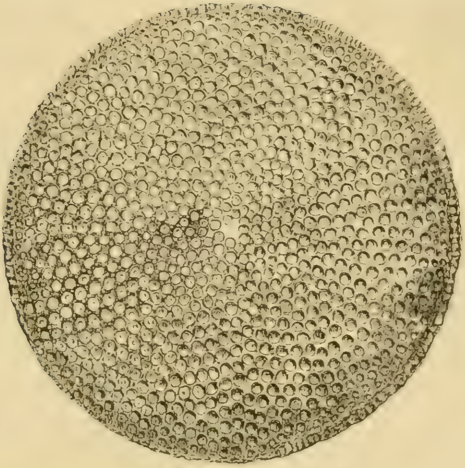
* 'Monthly Microscopical Journal,' vol. iii., p. 311.

† Ib., vol. iii., p. 104.



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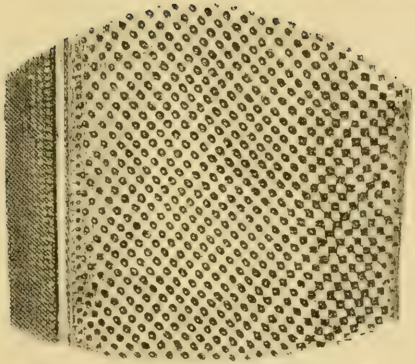

Portion of Pleurosigma Form[illegible]

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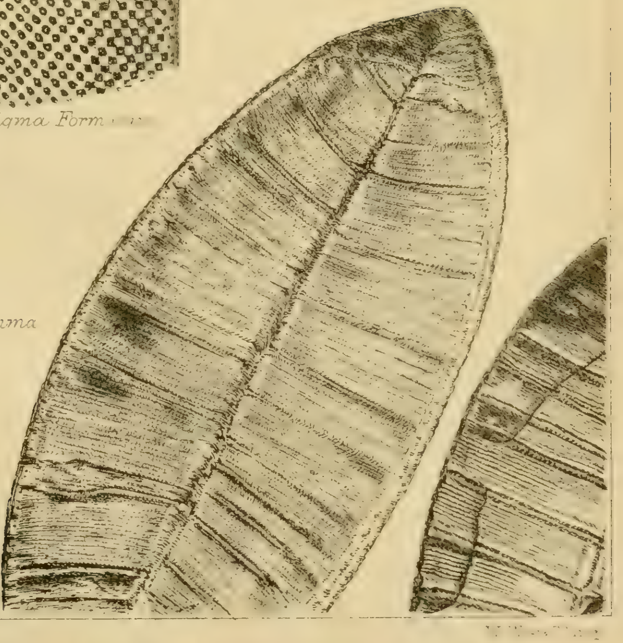

Surirella Gemma

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II.—*Microscopical Examination of Water for Domestic Use.*

By James Bell, F.C.S.

(*Read before the* Royal Microscopical Society, *March* 1, 1871.)

The microscopical examination of water for domestic use is, in my opinion, a suitable subject to bring before this Society. The subject is essentially a practical one, and affords a wide field in which microscopists might occasionally spend a portion of their time with much profit.

A sample of water which is largely contaminated with sewage, exhibits, when placed for a few days in a warm chamber, such a mass and variety of life, that the mind is almost bewildered in trying to master in detail the numerous creatures that have sprung into existence within so short a space of time. From the diversity of the ingredients usually composing sewage, it would, indeed, be difficult to find a mixture so well adapted for the development of a variety of organisms.

Mr. Heisch was, I believe, the first who attempted to point out a possible means of detecting, by the use of the microscope, whether water had been contaminated with sewage or not; his method being founded upon the development therein of a distinctive organism in the presence of sugar.

He found that when a small proportion of pure cane-sugar was added to water containing sewage, and the mixture was maintained at a temperature between 60° and 70° Fahr., and placed in a position where plenty of light was admitted to the bottle in which the liquid was contained, the mixture became turbid in times, varying from twenty-four to sixty hours, and contained cells and a delicate fungus mycelium, followed by the ultimate development of the odour of butyric acid. From this, Mr. Heisch concluded that sewage contained particular germs, which when brought into contact with sugar in water immediately began to grow.

This observation was necessarily regarded as a most important one, not only because it promised to effect a change in the present mode of determining the presence of sewage in water; but to lead to the discovery and identification of the germs which generate epidemics.

Mr. Heisch deserves great credit for initiating such an important line of investigation, and the combined researches of microscopists on the subject may ultimately lead to important practical results with respect to the laws of health, by determining the exact conditions under which the germs of various organisms are developed.

Shortly after the subject was brought before the Chemical

Society, I had occasion to assist at the examination of a sample of water obtained from a well in the neighbourhood of Drury Lane, and suspected to contain impurities. To a portion of the water Mr. Lewin added a small quantity of pure cane-sugar, and in less than twenty hours the mixture became turbid, and the liquid when submitted to a microscopical examination was found to be literally alive with minute bacterian-like bodies in an extremely active condition; and it was obvious that these little organisms by their action on the cane-sugar were the immediate cause of the turbidity. The water was bright, and there was certainly nothing in its appearance to cause any suspicion that it was contaminated with sewage. Subsequently it was ascertained that the drain-pipes of an urinal not far from the well were stopped up, and that the earth around the well was saturated with urine. On being informed of this fact, and recollecting that the characteristic fungus mycelium had not been observed when the water was submitted to the sugar test, I made various experiments with mixtures of urine, in different stages of decomposition, and New River water, and in every instance failed to discover any distinctive fungus mycelium, the field being almost exclusively occupied by bacterian bodies.

About six weeks after this I accidentally made the observation that water which had been passed through animal charcoal became turbid when submitted to the sugar test, and that a large development of bacterian bodies was produced. I had purchased a new charcoal filter in the month of September last, and I was naturally led to try the water by the sugar test before and after filtration, when to my surprise I found that the filtered water became turbid, while that which had not been filtered remained bright. This result at first puzzled me, but as I had observed last summer, during the course of my experiments on fungi, that phosphate of lime exercised powerful deteriorating properties by promoting the development of organisms, I was led to attribute the presence of the bacteria in the water after filtration to the action in some way of the phosphate of lime in the animal charcoal. The mystery of this action of the animal charcoal has since been explained, as it is known to be a fact that a portion of the phosphate of lime in the charcoal is dissolved out by the water when passing through it.

I have made some special experiments with phosphates in pure distilled water, under various conditions, and the results showed that in every case when phosphate of lime was present, the liquid, on the addition of cane-sugar thereto, became turbid and literally alive with bacteria, in times varying from twelve to thirty hours. The phosphates of the alkalies gave negative results, but when nitrate of ammonia was present, a slight turbidity occurred after the lapse of several days, and the liquid was found to contain a few bacteria. The results of these experiments confirmed my previous observations, that the phosphate of lime possessed powerfully stimulating properties in promoting the development of organisms. The phosphates of the alkalies, however, in the presence of pure vegetable albumen, also promote the development of organisms, and produce turbidity in cane-sugar solutions.

I also made various experiments with water passed through animal charcoal, and with water to which animal charcoal had been added, and in every instance when sugar was added, and the mixture placed in a chamber maintained at a temperature between 60° and 70° Fahr., the sample became turbid, in times varying from twelve to forty hours.

I have since observed that animal charcoal which has been in use for a considerable length of time ceases to possess the property of imparting to the water the power of producing turbidity on the addition of sugar; doubtless from all the phosphate of lime having been dissolved out by the long and constant action of the water.

Hitherto in the course of my experiments both with phosphates and urine I failed to obtain any characteristic mycelium; but it occurred to me that so long as the field was occupied by such a mass of bacterian bodies, there was but a small chance of obtaining the development of any other organisms, and as I had observed that the greater the proportion of urine or phosphate of lime present in the water, the water not only became turbid within a shorter time, but the bacterian bodies were greatly increased in number.

To obviate this I experimented with more dilute solutions. I first made an experiment with one drop of fresh urine in 20 ounces of New River water, and the result was completely successful, and I obtained an exquisitely beautiful filamentous development corresponding to the characteristic mycelium described.

Now I had observed that the bacterian bodies produced in New River water, when a phosphate was present, were identical in appearance and character with those which were produced by the addition of fresh urine to a portion of the same water, and it was difficult to comprehend how, when fresh urine was used, one class of the organisms developed corresponded with those produced in the presence of phosphate of lime, while the filament was absent.

To try and assimilate more the conditions of making the experiments with a phosphate and the urine, I added very minute quantities of phosphate of potash to New River water—from two to five hundredths of a grain to 40 ounces—and with this proportion of phosphate a beautiful filamentous development similar to that which was produced by the one drop of urine, made its appearance, thus clearly indicating that phosphate of lime exercised an important part in causing the development.

I next largely diluted some water that had been filtered through animal charcoal, and subjected a portion of the mixture of filtered and unfiltered water to the sugar test, when an abundance of fine filaments was developed. These filaments were not identical with those produced in the presence of urine; but the difference was probably due to my not having arrived at the proper conditions.

From what has been stated it will be seen how easily by a slight change of conditions the line of development of living things may be modified, and this is one of the great difficulties to be contended with in attempting to establish whether a water is impure or not, from the development therein of any particular organism; and it is even possible for a water under certain conditions to contain combined nitrogen without producing, when submitted to the sugar test, any turbidity.

Dr. Frankland, whose researches on this subject are full of interest, was the first to make the observation that effluent sewage water, when passed through a thick bed of gravel, was deprived of its phosphates, and that although containing combined nitrogen, the water, when submitted to the sugar test, remained bright.

I was unsuccessful until within the last few days in obtaining any filamental growth from vegetable albumen approaching in appearance the filaments that occur in water containing sewage. A small quantity of partially purified vegetable albumen was introduced into New River water contained in an open glass vessel and left for nine days, at the expiration of which time the water contained filaments, both in strings and fragments, the latter being almost undistinguishable from those found in effluent sewage waters.

While making the experiments with urine and phosphates I also made many with sewage and effluent sewage water under various conditions. There happens to be near me a farm on which the sewage of Epsom is utilized, and I obtained various samples of the sewage as it entered the farm, and of the effluent sewage water, and also samples of the latter after it had become mixed with the water of a brook.

In dealing with such samples as these in their normal condition, and examining them microscopically from time to time, the first thing that strikes an observer is their capability of producing a great variety of life without the addition of sugar or any extraneous substance, and when, after various degrees of dilution, the samples are submitted to the sugar test, they become less or more turbid, and contain variable proportions of fine filaments and bacterian bodies.

In effluent sewage water, I have rarely met with the filaments in that beautiful network form in which they are found in water containing only urine or a phosphate; but there is no difficulty in deciding that both filaments belong to the same class of growth.

The filaments met with are of at least two sizes; one is so fine that the joints cannot be seen when magnified 1000 diameters. The joints in the other filament can be observed when magnified about 400 diameters. Both descriptions of filaments are found in water containing sewage, but in some diluted samples of effluent sewage water I have only been able to discover the finer filaments, while in others only the larger. In mixtures of urine or of a minute quantity of alkaline phosphate with New River water, the larger filaments are found, and they occur in the network form.

Both filaments soon break into pieces of various lengths and diffuse themselves throughout the liquid. In the case of the filament produced in the water containing urine, I observed that in its earlier stages of development the filament appeared to be surrounded by a sort of soft casing, which gradually separated from the filament, the jointed character of which was then readily distinguishable.

While dealing with the subject, it is proper to point out that when some descriptions of ditch and other dirty water are submitted to the test, filaments similar in character are developed, and after these break up and become diffused throughout the liquid, they are hardly distinguishable from those produced in water containing sewage; in fact the only difference apparently observable is that the broken filaments are much less numerous than in samples of effluent sewage waters, which undoubtedly produce the filamentous growths in great abundance. The dirty waters upon which I operated became turbid, and were found to contain bacterian bodies.

The rapid growth of the filaments in the presence of phosphate of lime and cane-sugar is very curious and interesting, and the interest in their development is somewhat heightened by the fact that the filaments will, I think, be found to belong to a higher order of vegetation than fungi, *viz.* to the Algæ and probably to the class Oscillatoriaceæ.

In samples of water containing sewage a large proportion of the bacterian-like bodies partake more of the character of vibrios than the corresponding organisms in water containing phosphate of lime, and free from organic matter; but the line of distinction is so fine that I could not undertake at present to define the difference. Those bacterian bodies which are produced in water containing a large proportion of sewage, and especially urinary sewage, appear to be more flexible than those developed in the presence of sugar in effluent sewage water, or in water containing phosphate of lime only. The most active condition in which they are found, is when they consist apparently of two cells or divisions, and in this form they are extremely rapid in their motions, and are seen darting constantly across the field, and occasionally stopping and performing a revolving motion. I cannot help thinking that these bodies, which multiply with such extreme rapidity and produce such rapid changes in solutions of organic matter, are a dangerous form of organism to be taken into the human system.

Finally, the results of my experiments are in accordance with the theory, now almost universally accepted, of the general diffusion of germs; and once a suitable soil is produced for any class of these germs they will soon find it out and begin to grow, just as bacterian bodies develop in New River water immediately after a small proportion is added thereto of phosphate of potash and cane-sugar respectively.

III.—*On the Winter Habits of the Rotatoria.*

By Charles Cubitt, C.E., F.R.M.S.

(*Read before the* Royal Microscopical Society, *March* 1, 1871.)

Plate LXXXI.

Every step we take in the investigation of the habits and functions of this interesting Class of the animal kingdom leads us to the conclusion that while a reliable classification is still wanting, facts are being daily established on many hitherto doubtful points in their economy which are tending towards the establishment of a reliable basis for such a desirable end.

I have for some successive winter seasons directed my attention to these particulars as far as the tube-builders are concerned; and as it appears from the published records of kindred societies that much misapprehension still obtains on certain points in the economy of this interesting CLASS, I beg leave to preface the consideration of the subject proper by a few prelusory remarks on the points in question.

It will consequently be necessary for our purpose to distinguish the members of the Loricated Families of SECTION I. of the CLASS ROTATORIA of Ehrenberg's SYSTEM under two Divisions,—the Solitary Fixed, which include *Melicerta*, *Limnias*, *Œcistes*, *Tubicolaria*, *Stephanoceros*, and *Floscularia*; and the Clustered Tree, which contain only *Lacinularia* and *Conochilus*; for while these disagree in points of habit they are identical in an important point in their anatomy, which by careful and persistent investigation I have proved and recorded.* They, one and all, whether ciliated or setigerous, possess a secondary range of vibratile cilia situated anteriorly to the mouth, the marginal wreath being constituted to act as a foraging organ, in collecting a heterogeneous mass of particles about the disk; but the secondary range, acting in conjunction with certain ciliated lobular process, exercise a discriminating function in selecting from the heterogeneous mass, and in separating therefrom, and severally disposing of the particles so selected for alimentary or building purposes, and in rejecting others for the time being, which are brought again and again under the influence of the marginal wreath by means of the cements which that creates.

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* 'M. M. Journal,' May, 1870.

Dujardin's classification embraces each of the members of the foregoing Division, both Fixed and Free, but it includes a form which is not loricated, *viz. Ptygura;* this, however, could not be admitted, as its identity as a mature animal is doubtful.

We find, however, that abnormal conditions obtain with certain members of the Solitary Fixed Division, which normally are *permanently attached*, particular instances occurring in respect to *Floscularia* and *Œcistes*, which under such conditions occasionally forsake their first-selected habitats, and resort to foreign ground, where they re-establish themselves, and construct anew their so-called lorications, simply gelatinous or floccose, according to their respective characters; but whether these abnormal conditions are accidental or natural I have some hesitation in declaring; I consider it, however, worthy of note, for we shall see as we proceed that such a capacity in these forms associates them closer than hitherto acknowledged with certain members of SECTION II.

Each individual of the Fixed Division is furnished with *two* setigerous antenna, situated bilaterally, but not diametrically opposite; hence the great difficulty in arranging animals for observation precisely in such a position that they can be viewed without the interposition of some other part of the body, and not even then with inferior appliances, either mechanical or optical; but with good apparatus and patient application they are to be found in one and all: they are clearly apparent in *F. coronetta* (*mihi*), as also in *Stephanoceros*, and familiar to all observers in *Melicerta*. It must therefore be distinctly understood and accepted that no Rotifer exhibiting a *single* setigerous antenna can be admitted as a member of the Fixed or of the Free Division of the Loricated Families of the Rotatoria.

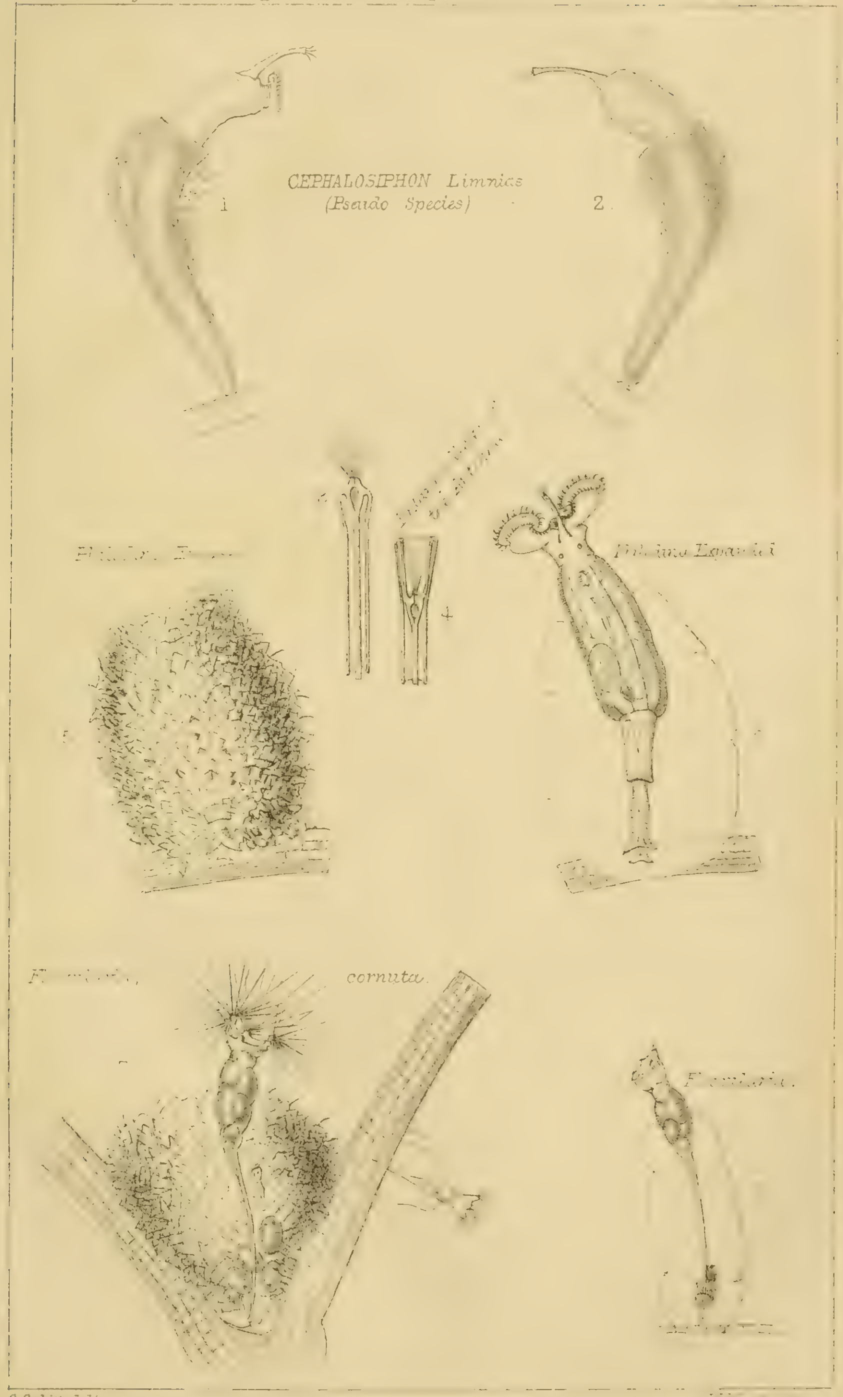
It now becomes necessary to distinguish certain members of SECTION II. of the CLASS ROTATORIA, the so-called Illoricated Family Philodinæa, which by their peculiar habits and structure form a connecting link, hitherto unrecognized between this Family and those of SECTION I. previously referred to; for our present purpose, therefore, we must separate these also into two Divisions, which may be designated the Temporarily Fixed and the Absolutely Free, this last being only represented by *Hydrias*, *Typhlina*, and *Monolabis*, which under a new classification should be included with the numerous members of the following Families,—Hydaliniæa, Euclanidota, and Brachionæa, the Flexible Creepers of Gosse.

Now, this Temporarily Fixed Division is represented by *Callidina*, *Rotifer*, *Actinurus*, and *Philodina*, which though nominally Free are specially organized to permit them to assume at will, either a Free or a Fixed position, and of maintaining these positions respectively for periods of indefinite extent. The foot of each individual of this Division is furnished with a *sucker* extending beyond the *toes* of the telescopic footstalk, which together constitute a mechanical support that may be exercised or released at will.

This Temporarily Fixed Division, while it assimilates through certain members of SECTION I. by reason of the vagrant habits of the *Floscules* already referred to, differs, moreover, still further from the Absolutely Free Division in one most important particular, *viz.* the possession of a *single* setigerous antenna which distinctly removes it not only from the Free Division possessing no such appendage, but furthermore from the Solitary Fixed forms of SECTION I., which possess unexceptionally two antenna; so that the pseudo genus *Cephalosiphon* with its specific title *Limnias* of Ehrenberg's Classification (Plate LXXXI., Figs. 1 and 2), should not be admitted into the Family in which it is unwittingly placed. It belongs indisputably to this Temporarily Fixed Division, and is generically a Philodine under conditions which at first might appear abnormal, but which prove on a more intimate acquaintance to be natural functions.

The *Philodines*, moreover, are provided not only with the secondary range of cilia which characterize the Fixed Division, but also with a ciliated rudimentary *pellect-cup* in conjunction, which they employ in building a floccose covering round about them, and this conclusively associates them with this Division. With regard, however, to the remaining members of the Family Philodinæa according to this proposed Division, I am not at this time prepared to speak, and can only suggest it as a point for investigation worthy of the attention of my fellow-workers.

I have had numerous examples under observation throughout the winter months for some years past, having broken the ice to obtain them. Placed in a cool situation, they invariably remained some hours and even days in this completely loricated condition; but as the warmth of the room, and that of the lamp while under observation, began to be appreciated, palpable movements were first manifested within the floccose covering, the orifice of which is closed by a coalescence of the materials composing it, such as described by Mr. Henry Davis, in reference to *Œcistes longicornis*: they eventually, after shorter or longer periods of repose, wriggled out of these temporary dwellings, declared themselves *bona fide Philodines*, and swam freely and buoyantly around the field, exhibiting distinctly their bilobed rotary organs, supplemented with the secondary range of appropriating cilia, with the conspicuous



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Winter Habits of the Rotatoria × 100.

single setigerous antenna, practising as is found on closer examination an incessant protrusion and retraction, and thus manifesting the same general construction as that so lucidly described by Professor Williamson in his elaborate description of *Melicerta ringens* in 1853, and it is seen in the instance of *Philodine* that when the tentacle is protruded it is terminated by a tuft of fine divergent setæ (Plate LXXXI., Fig. 3), implanted on a pear-shaped body, "from the under-side of which there proceeds a delicate muscular band" which in contracting draws this pear-shaped body downwards, thus inverting the extremity of the tube, and forming a double sheath protecting the setæ (Fig. 4). And such I must submit to the process followed in the instance of *Œ. longicornis.* It is opposed to the facts of animal physiology that a vascular structure like the tube, should support a palpable diaphragm pierced with an orifice presenting a sharp edge such as would result from drilling a hole in a metal plate.

From the opportunities I have had of examining many examples of the building propensities of the genus *Philodina*, I can confidently assert that these temporary dwellings are more frequently of the consistent form shown by Fig. 5, with the enclosed occupant indistinctly indicated, and in outline by Fig. 6, which is offered as a portrait of this vagrant occupier in her expanded condition; and I am prepared to affirm that these forms do not abstain altogether from this habit of building, even during the warmer months; they perform the same operations under conditions of greater activity, not remaining on one spot for periods of sufficient duration to perfect the work, as shown by Fig. 5, but only to produce effects similar to those shown by Figs. 1 and 2, which are reproductions of Mr. Tatem's sketches.

The Floscules also give conclusive evidence of this adoption of a winter protection. I find numerous instances of every species in which they have invariably selected the fork of the weed for the purpose, affording as it does additional security and stability for such a retreat. The inhabitants are generally accompanied by several eggs, and these, from the effects of the artificial warmth of a living room, are rapidly hatched, two or three at one time being frequently seen struggling for ascendency to attain freedom, so that I have had a numerous juvenile population of *F. cornuta*, which species happens this year to predominate in my collections. I have consequently selected this charming little creature, with its ova and family, as an appropriate illustration of a Floscule in its winter habitation, through which it has emerged under the effects of artificial warmth (Fig. 7).

This same class of protection is adopted by *F. campanulata* and *F. ornata* to a proportionate extent; and my new species, *F. coronetta*, which is the most sensitive and timid of them all, carries out this earthwork system to a much greater extent proportionately with its size. *Stephanoceros* takes similar precautions, though on the other hand inversely proportionate with its increased dimensions in comparison with its smaller compeers. *Melicerta* and *Limnias* simply draw a mass of flocculent particles into the mouths of their tubes.

There obtains amongst the Floscules a particular member, and so far as I am informed is nameless, with which I have had similar evidence of its winter habitation. It is somewhat rare, and is smaller than any one of the other members. The first specimen that came under my notice gave me the erroneous impression, from the undeveloped disk, the almost total absence of setæ, and its diminutive proportions, that this was the *male*, as I had on the first occasion failed to notice any ova either deposited or contained in what proved subsequently to be the ovary and not the testes as I had at first assumed, the contained matrix of finely granulate matter exhibiting on higher magnification the well-known nucleated nuclei. It is needless to add that they soon declared themselves in their true nature, by collecting at the foot from three to five fully-developed ova, as shown by Fig. 8.

It is satisfactory to find corroborative evidence in support of these views on the winter habits of the Rotatoria from facts which have lately been brought to light, showing that reverse conditions produce adverse effects—Mr. Chantrell, of the Liverpool Microscopical Society, having recorded instances in which with the Flosculariæ found in the Windsor ponds which contain *warm water*, that " the tubes disappear in the mature animals." This statement in its entirety must, however, be received with due caution, for, unable to detect the secondary wreath of cilia on the lobes of the tube dwellers, which is an indispensable attribute in their special organization, he may, with the same appliances, optical or mechanical, be equally incompetent to detect the transparent tubes, which, while devoid of foreign accessions, may nevertheless be present.

IV.—*The Magnifying Power of the Microscope.*

By Count Castracane.

"*Surirella gemma* may truly be called a *touchstone* for a high-power objective. I do not think that it has been clearly defined as yet."—John Mayall, 'Q. J. M. S.,' July, 1869.

The following paper, which I have just received from Fano (Italy), will answer, I hope satisfactorily, the above-quoted remark. It is an original, yet unpublished, MS. of Count Castracane. I did my best to translate it faithfully from the Italian. That being my native language, it may easily be presumed I understood it right enough, though it might be a quite different case with my not home-made English. However, the benevolent reader will easily overlook any un-English phrase for the sake of the valuable matter.

P. J. Gagliardi.

Count Castracane's Paper.

(*Read before the* Royal Microscopical Society, *March* 1, 1871.)

Far from wishing to sit down as judge on the controversy, which lately sprung up between the illustrious American micrographer, Charles Stodder, and Dr. Hagen, of Cambridge College, U.S., about American microscopes and their respective value, I simply beg to offer them some suggestions, whilst I shall bring forward a few facts, wherewith I am perfectly acquainted by my own experience; these I hope will serve, better than any other argument, to clear up the mooted question.

Mr. Stodder[*] maintains against Dr. Hagen:—1. That the *Surirella gemma* given by Hartnack with *flat* hexagonal markings is simply a theoretical diagram of the said diatom. 2. That the hexagons were not probably seen, but with Tolles' $\frac{1}{12}$th by Mr. Bicknell. 3. That Mr. Hartnack himself did not see those hexagons but with his No. 10 ($\frac{1}{16}$th), and did not succeed in showing it to more than two distinguished micrographers. 4. That Dr. Eulenstein failed to see those hexagons with Nos. 10, 11, and 12, nay, even with the best of Ross, and the most powerfully constructed up to this time, the $\frac{1}{50}$th of Powell and Lealand.

In regard to the *Surirella* represented by Mr. Hartnack, in his special memoir on this subject, with *oblong* hexagons (*oblong* I think is a more exact expression in this case, than *flat*), I may say that since the 7th March, 1869, I had myself the occasion of treating this subject, in a short memoir, where I tried to set right what I deemed wrong.[†] There, speaking of the difference between preparations made on balsam and without it, I observed that in the first case the *striæ*, or better, the longitudinal divisions of *S. gemma*, when observed in an oblique, monochromatic light, present a wicker-basket texture, and make us believe that Hartnack was right when he interpreted and sketched them like oblong hexagons. But in the second case in which, for Möller's test-plate prepared on balsam, I substituted dry shells, simply covered with a thin glass, they presented immediately nice moniliform striæ, or rows of small spheroids, so disposed, that the polar axis of each of them was equal to $\frac{2}{3}$rds of the equatorial. I may add, moreover, that, although the difficulty of distinguishing the most minute particularities of a diatom prepared on balsam be far greater than that of seeing it in its dry condition, I did even succeed, after a little pain and trouble, in perfectly making out the spheroids in Möller's own preparation. And more than that, under a properly arranged monochromatic light, their appearance did turn out so evidently clear, as to leave not the least doubt or perplexity about the right interpretation of such wonderfully minute details. This I could not see perfectly till late, except with a good No. 10, of Hartnack; but this year I obtained the very same result with another No. 10, which Mr. Nachet was kind enough to construct purposely for my trial.

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[*] See 'Monthly Microscopical Journal,' Nov., 1870.

[†] See 'Atti dell' Academia dei Nuovi Lincei,' § iv. Su l'uso delle Linee di Nobert, etc.

The nature and form of the structural details of that interesting diatom being thus set aright, it is plain at the same time that Mr. Bicknell was not the only one who could witness the structural features of the *Surirella* in question. His perception, besides, having been not quite true to the fact, though his shortcoming was very excusable, as some of the most distinguished micrographers were liable to the same mistake for several years, taking for hexagonal the spheric markings of *Pleurosigma angulatum*. I had thought so myself at first, deceived by imperfect photographs, and backed, as I was, by the authority of first-rate micrographers who had preceded me in such trials. But I was soon put right, first by better photographs, which I obtained more clear and accurate with the exquisite F objective of C. Zeiss, of Jena; and then again by wonderfully increasing powers which Colonel Woodward was kind enough to send me, for which I publicly express my gratitude, waiting the first opportunity to return it by some of the best photographs of my own collection.

Thus my successful studies were happily performed by means of Hartnack and Nachet's No. 10. Yet whoever does make use of these valuable objectives with a simply white light cannot expect to obtain more than the mentioned wicker-basket appearance, not the precisely moniliform structure of *Surirella gemma*. More than once Hartnack himself showed me the said diatom under this sort of light, not only with his No. 10, but even with Nos. 9 and 8, holding as his private opinion that with his 7 merely he might have had the same result. After this I can hardly understand how Mr. Hartnack, so skilful as he is in managing his own objectives, and so familiar with the diatom in question, could have failed to show two distinguished micrographers the very same details, unless we assume that one person's eye is more apt than another to descry such utterly minute features.

Neither could I think otherwise in regard of Stodder's assertion that the most distinguished German diatomologist, Dr. T. Eulenstein, failed in making out the same details with Hartnack's Nos. 10, 11, and 12; nay, with the strongest objectives of Ross, even with the highest at present known, the wonderful $\frac{1}{50}$th of Powell and Lealand. Such negative result does evidently contrast with my own experience. If it might not be pleaded that more urgent business perhaps prevented the great microscopist from giving his precious time to the troublesome and not very easy task of examining so finely minute structures, which, though they do not considerably advance our diatomological science, have certainly a great importance for testing the relative value of our microscopes; the increasing power of which prompts us to even more delicate researches, by which the sphere of our knowledge is more and more enlarged.

On this subject I read in the Monthly Microscopical Journal (Nov., 1870) that the illustrious Professor Huxley, at the meeting of the R. M. S. (Oct. 12 that year), lamented the actual insufficiency of our microscopes for histological researches, saying that "practically the nature of the question just now is whether, in an organic tissue, one could truly define a point not more than $\frac{1}{50000}$th of an inch in diameter;" adding again, "Histologists, I fear, are at the end of their work unless . . . . they could obtain microscopes which would enable them to separate two points the 100,000th of an inch apart."

To this I might confidently answer that what the illustrious Professor thinks to be future has been attained already. In proof that this is no vain presumption of mine, I adduce the following reasoning:—Whoever takes any interest in the improvements which are daily made in the microscope will surely have read of the various articles and memoirs that, during these last three years particularly, have been published in both the Quarterly and Monthly Microscopical Journals by the most active and zealous microscopists. Nobert's wonderful incisions upon a glass plate, disposed in thirty graduated bands, till the last reaches $\frac{1}{3544}$th of a millimètre apart, seemed at the beginning to baffle the highest powers of the most perfect microscopes. Yet at the very beginning of 1867 I had the satisfaction of resolving those minute striæ to the very last. I know that Mr. Nobert, after he heard that the microscope had met his challenge, increased his divisions till he reached the $\frac{1}{4430}$th. I have not had the opportunity of putting my microscope to this new test, though I feel confident to reach it also. However, we have heard that in America Drs. Woodward and Curtis did already succeed in resolving those *minutissimæ striæ*; and to prove that what they have seen was no delusion or hallucination, they have got exact photographs of them, copies of which they sent over to Europe.

After this, I think we may safely assert that, even as it is now, the microscope has fairly reached—nay, we might say has passed—the line which Professor Huxley deemed indispensable for his histological purpose.

Another proof I might adduce in confirmation of what I have stated—that is a photograph, which I have obtained last year, of *Amphipleura pellucida*, out of Möller's graduated test-plate, notwithstanding the difficulties which attended that preparation on balsam. Unfortunately my negative was blurred and rather faintish, so that it could not give good positive images. Nevertheless, the striæ are there so finely and so distinctly drawn out, that they may be perceived clearly enough, though the magnifying power of the microscope was not higher than 640 diameters. The matrix gives without any doubt 4000 in a millimètre, which answers completely Huxley's desideratum of one hundred thousandth of an inch. It was Hartnack's No. 10 that gave me at first that photograph, the illumination being taken from a decomposed sunbeam. But I had afterwards the same result with the same number of Nachet; both, doubtless, first-rate objectives, since they stood the most difficult trials. However, as I am desirous to advance scientific inquiries, rather than to boast of my own instruments, I dare say that, should Professor Huxley or any other student of the secrets of nature, try the same thing with microscopes of the best make, either European or American, using the same precautions which I did, they would hardly fail to obtain the very same results.

My practice is this: whenever I want to make out some of the minutest details of any organism, or to get over any difficult test, and I see that my microscope, after all due preparation, and with the best prospect of light, fails to answer my expectation, I refer, as a last resource, to my prism, and get from it a coloured sunbeam. Blue or green are the colours which I prefer; they are the most suitable for the purpose.

The elimination of every, even the slightest, chromatic aberration obtained by this means increases, in my opinion, the defining and penetrating power of the microscope, and enlarges its dominion on the field of observation. Different other means have been now and then suggested, such as an alcohol light saturated with chlorine of iodine, or a light passed through a stratum of cupro-ammoniacal solution, or even through a glass of cobalt: all these lights may be very useful and for some special purpose even preferable to any other, as

Dr. Woodward observed, speaking of photography; but for direct observations with the microscope, the effects obtained by them are by no means to be compared with the marvellous results of a monochromatic illumination.

And I do not think it absolutely necessary for this purpose to have recourse to a beam of the *sun*, which in many countries less favoured than Italy is not rarely a mere desideratum, and very often a dim, cloudy thing.

A brilliant luminous point of electric light—a light obtained from oxhydrogenic flame—acting upon lime, magnesium, or zirconium, perhaps also the magnesium-wire lamp, may supply the deficiency of the sunbeam. Each of these, simply white lights, decomposed through a prism, will give a monochromatic illumination sufficient to reveal the best structural details, which up to this day baffled the keenest researches of the student.

[The following note has been sent to us by Professor Joseph Gagliardi, who received it from Count Castracane, and we print it here because it has reference to the foregoing paper.]

"As I am now settled in my new residence (near the Capitole), I began three days ago to work in earnest. My newly self-imposed task is to examine the *Pinnularia* genus, in order to ascertain if the ribs (coste) or pinnules of the different species be not truly continuous, but rather a series of dots close pressed together, in which case the distinction between the two genera *Navicula* and *Pinnularia* ought to be superseded. From this very moment I may say, without fear of mistake, that *P. oblonga* is distinguished with riblets (costole) made up of a series of small lines (lineette) parallel one to the other, and extremely minute—I was able to value their number to 4160 to a millimètre. Nevertheless, I cannot yet say I have recognized any discontinuity in the pinnules of *P. nobilis.* As soon as I shall have leisure to study with due attention, and with the monochromatic illumination, the principal types of the said genus, I shall publish the result in a special article. . . .

"Yours affectionately,

"FRANCIS CASTRACANE.

"ROME, 18*th February.*"

V.—*A Few Experiments bearing on Spontaneous Generation.*

By Metcalfe Johnson, M.R.C.S.E., Lancaster.

Since Professor Frankland's careful repetition of Dr. Bastian's experiment on spontaneous generation still leaves the matter more decidedly in favour of the "ab ovo" theory, which I attempted to support in 'M. M. J.,' November, 1870, I am inclined to hope that the following experiments will still seem to point in the same direction. The practical importance which the subject is taking in reference not only to the preservation of commercial products, as meat, preserves, fish, wines, drugs, &c., but in its bearing upon epidemic disease, render acceptable any communications that can in any way throw light upon the subject. I therefore trust this may not be devoid of interest.

One wine merchant of my acquaintance tells me that he washes his bottles out with Condy's fluid and water, and finds his wines (especially claret) to keep better for it. He also tells me that when opening a bottle of light wine, such as hock, claret, or sauterne, which is not to be consumed on the day of opening, that he burns a small sulphur-tipped match in the neck of the bottle, by which means the wine is preserved from acetous fermentation for about a week.

Aug. 12*th*, 1870.—I boiled a pint of distilled water in a Florence flask, and whilst at boiling heat inserted 20 grains of tartar emetic. This caused a new ebullition, as if some change was taking place either in the water or the salt. The bottle being quite full, I inserted a conical cork, and sealed it tight.

Aug. 13*th*.—In a wide-mouth stoppered bottle containing 23 oz. of air, I put 8 oz. water and two cubic centimètres of a solution of potassic permanganate, containing 1 grain to every 10 centimètres of water. After shaking for a few minutes, I inverted the stoppered neck into a cup containing water and permanganate. The bottle was afterwards frequently shaken.

Aug. 19*th*.—I filled some 2-oz. clear glass bottles with the liq. antim. tart. made Aug. 12th. I then displaced half the contents of the bottle (1 oz.) by the air washed with permanganate, and marked the bottle A. The second bottle, B, was served in the same way. In C, I burned a small piece of sulphur in the air contained in the upper half of the bottle.

D had 10 drachms displaced by air which had been washed with carbolic acid.

In E, 1 oz. was displaced by air in which carbolic acid had been volatilized.

In F, 1 oz. was displaced by air which had been washed with an aqueous solution of sulphurous acid.

These bottles were all corked tightly, and sealed over, so as to prevent access of air.

I find in my notes, Sept. 25th, 1870 :—

A has a sediment of leptothrix (?) at bottom. This did not form so soon as the others.

B, several leptothrix plants (?) formed early.

C has a plant of mucedo, GREEN, floating on the top; a dense white sediment at bottom. This liquid was quite clear for *ten* days after it was put up.

D, copious deposit of filaments.

E, a plant of leptothrix (?) at bottom.

F, a small plant at bottom.

I subsequently examined them under the microscope, and found in A, *branching* leptothrix filaments containing spores in many of the tubules, but surrounded by innumerable spores *still*. B the same as A. C, beaded filaments (green to the naked eye) on a mass of white leptothrix filaments.

Feb. 5th, 1871.—A and B contain leptothrix filaments and spores. The bottles have no smell.

C is a mass of beaded filaments on a plant of leptothrix. The bottle smells musty.

D, no smell; small filaments; few spores.

E has not been opened since Aug. 12th. It now contains a white film at bottom, composed of small filaments, not containing spores in tubules, but a few composed of larger beads, bifurcating to form two larger heads.

F, like E, but smaller in quantity.

For comparison, 1 oz. of the liq. ant. tart., made Aug. 12th, was put into a 2-oz. bottle, and corked up—half air; and on Aug. 17th leptothrix filaments began to appear.

Oct. 6th, 1870.—Dissolved 50 grains of bichloride of mercury in 10 oz. of distilled water.

Prepared a solution of iodide of potassium, of which 10 cubic centimètres precipitated the red iodide of mercury from 1 oz. of the above solution.

Dissolved 5 grains of tartar emetic in 1 oz. of the liq. hyd. bichlorid. Then added 10 cc. of the solution of iodide of potassium, filled the bottle up to 1½ oz. with distilled water. Corked the bottle, leaving a small bubble of air. Tied the cork down very tight, and covered the whole of the cork and neck of the bottle with sealing-wax. The red precipitate soon settled down to the bottom of the bottle. On opening it, Feb. 5th, 1871, for examination, no trace of filament or spore could be found.

At the same time an ounce and half of water, warm from the still, was put into a bottle with 5 grains of tartar emetic. It was corked and sealed as before; and on Feb. 5th, 1871, when opened for examination, no trace of any filament or spore could be discovered.

Feb. 7th, 1871.—Examined the solutions, and found in the distilled water several globular spores, one elongated so as to form a filament, and a small bunch of four filaments with a few spores attached. The bottles have been placed, open, in air and light.

I examined also the bottles containing the iodide of mercury, but found no traces of organisms.

Feb. 11*th.*—Examined the solutions again. There is no decided leptothrix formed in any of them. In the distilled water I found a few spores, all round—one composed of three attached in a line.

In the liquid containing iodide of mercury I found some oval angulated bodies, the nature of which I could not decide. They were soon obscured by the crystals of tartar emetic forming by evaporation.

Feb. 16*th.*—Found a large plant of leptothrix at bottom of bottle containing antim. tart. in distilled water.

No leptothrix or spores in the bottles with iodide of mercury.

Oct. 6*th*, 1870.—Put some tartar emetic into a bottle with *spring* water, and a second with distilled water, half air. In both cases leptothrix filaments were soon formed.

In a bottle of precipitated bichloride, with iodide of potassium, some tartar emetic was dissolved, half the bottle was filled with air, and lightly corked. In this case, on Feb. 5th, 1871, no change has occurred.

Two drachms of citric acid were dissolved in 6 oz. of water, to which was added about a drachm of Condy's fluid. The colour soon disappeared, and in course of time (10 days or so) leptothrix appeared.

Oct. 17*th.*—Placed the liver of a rat (fresh, and washed in distilled water) in a 2-oz. bottle of distilled water; inserted a cork, and covered the whole with *cotton-wool.* I then placed it in a dark cupboard. In seven hours the water below it was red with blood globules.

Oct. 28*th.*—Removed the cotton-wool, and found a proligerous pellicle, consisting entirely of small paramœcia, or monads, in an elongated condition.

Oct. 29*th.*—Examined the proligerous pellicle, and found a few paramœcia, but the mass consists of millions of moving pin-point monads.

These experiments point to certain questions of interest relating to spontaneous evolution from inorganic matter.

The first, in which air washed with disinfectants of various kinds was introduced to bottles containing a solution of tartar emetic. In none of them was the growth of Mucedinæ prevented, though in all it was longer in making its appearance than in unwashed air. (If this were the fitting place I could show how this is borne out by my experience in the sick room with regard to the use of disinfectants.)

A second point to be noticed is the formation of the green mucedo in the bottle to which sulphur vapour had been introduced, which seems to me to have an important bearing on the question of Xenogenesis. In all the cases, I conclude the germs must have been in the liquid, but not in the air above.

In the second experiment with bichloride of mercury, if germs are living they would be destroyed by this corrosive, and if produced out of the inorganic C H O N of the tartarate, they might still arise by spontaneous evolution; as all the corrosive sublimate was again removed by precipitation by the iodide.

In the case of the distilled water solution, if the filaments arise out of the inorganic C H O N, here was an opportunity for that to take place. But we find no change, as there were no germs in the water, none in the salt, and no air for them to arise from.

In the experiment with citric acid, the mucedo is formed in spite of the Condy's fluid; which at once indicates that this disinfectant is not fatal to all forms of protozoal life. In 1866 I found that neither *Daphnia pulex*, the larvæ of gnats, nor minnows, were at all inconvenienced by Condy's fluid, while carbolic acid speedily killed them all.

The experiment with the liver of a rat seems to show that *cotton-wool* is not a safe protector against "germs"!!

As one who has looked anxiously for motion as a sign of life in the "pin-point" monads, especially as distinct from the current and Brownian movements of inorganic particles, I cannot but feel much sympathy with the remarks in this Journal of Feb., 1871, pp. 81–2, "On Motion in Microscopic Granules," quoted from C. S., in the Boston Journal of Chemistry. The remarks in Professor Frankland's paper in 'Nature,' Jan. 19, 1871, "On the Movements of the Dumb-bell," particles of glass (which might have led to error in less experienced hands than those with whom he was associated) show how necessary it is to be cautious in minute examinations. I have myself called attention, in a note, 'M. M. J.,' April, 1870, p. 200, to the movements of sodium on water when ignited, which resemble those of some forms of monads. I would venture to suggest that when movements of a particle or object are referred to as a proof of vitality, some detail of description should be given as to the modes in which such movements occur.

As a rule there is much difference between the cyclical movement of a small monad, and the slow, irregular, jerking movement of particles, which is due to evaporation of the liquid, and the regular, synchronous, and similar-direction-movements of particles in a current, or those movements which are seen in examining infusions, and produced by the action of some moving animalcule which is outside the field of view.

NEW BOOKS, WITH SHORT NOTICES.

Microscopic Objects Figured and Described. By John H. Martin, Honorary Secretary to the Maidstone and Mid-Kent Natural History Society. London: Van Voorst. 1870.—Apart altogether from our desire that Mr. Martin had not written this work, we must express our regret that the publisher has sent it to us for review. We say this because we suppose he insists on a notice, and it is not in our power to say a single syllable in praise of the volume. It is without any aim; it can serve no purpose; and it is altogether the worst thing of the kind that we have ever seen. There is not, in the whole collection of plates any one which is even fairly passable, and there are many which are as truly execrable as it is possible to conceive. There are 194 drawings of various objects without one that we can say is fairly executed, and indeed some of them are so abominably handled that it is a matter of surprise to us that the author—be he possessed of the smallest possible experience—should have allowed them to appear. But, apart from this, the book is altogether aimless. Every one, or nearly so, of the objects (with the exception of some of Mr. Forbes's specimens at the end) have been done, and exceedingly well done, before, in various treatises on Natural History and Histology. And even the rock specimens have been in part done, and very admirably so, in an article some years ago by Mr. Forbes, in the 'Popular Science Review.' We have never beheld such abominable misrepresentations as the plates, for indeed there is not the faintest depth in them; there is a horrible flatness about them which gives one the idea of an ordinary drawing crushed out flat, so as to effectually remove any traces of perspective. We should gladly have avoided saying anything about the work at all, but it would be unfair to our readers to pass any but a most unfavourable critique upon work which has been so execrably handled.

The Natural History of the British Diatomaceæ. By Arthur Scott Donkin, M.D., Lecturer in Medical Jurisprudence to the University of Durham. London: Van Voorst. 1870. Part I.—This is the first part of what, when it is completed, will be an extremely useful book. But we fear it will not be finished within the time that the author first proposed. The plates are by Tuffen West, and of course are good; but somehow or other they do not appear to be the best work of that skilful artist. It may be that, as we judge from the cheaper edition, which alone the publisher has sent us, we may do the artist an injustice, but certainly the specimens which we have seen do not convey the idea that they are worthy of Mr. West's hands; still, they are very fair plates, and as they contain no less than forty-one figures, they are good, considering the price at which each part is sold. The author's work is, so far as we can see, well done, though he has not modified his statements in correspondence with the modern idea of a diatom. For example, he still describes the *naviculæ* as being striated, instead of being composed of a series of hemispherical nodules. He considers that he is justified in maintaining the old description, because the microscopes employed by "naturalists" do not exhibit the peculiar spheres of silica!! This is, it seems to us, a mistake, but of course it is a trivial one. The synonyms appear to be fairly stated; but we fancy that the distribution might in some cases be a little more fully given. On the whole, we may fairly say that the work is well done, and we may hope that the future parts will, if anything, exceed in value the present one.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Microscopic Characters of certain Rocks.—Geologists are gradually becoming more accustomed to the microscope in the practice of their profession. It is becoming essential for every geologist to use this instrument when possible in the examination of rocks. We have before us a reprint from the 'Philosophical Magazine' (February) of a paper by Mr. J. Arthur Phillips, F.C.S., on the microscopic character of certain Cornish rocks, which affords ample testimony of the value of the instrument. The microscopical specimens on which the author speaks have been prepared in the usual way, but the softer varieties of killas have received exceedingly careful manipulation. In the case of the more fissile specimens, it was often found very difficult to obtain satisfactory sections at right angles to the cleavage-planes; and even when procured, great care was required in mounting, to prevent their entire disintegration in the balsam employed for that purpose. Each section was examined by the aid of two different arrangements of the instrument. The first, spoken of as "a low power," magnified 60, and the second, mentioned as "a high power," about 400 linear. The polariscope could be conveniently used with both. Higher powers were sometimes, but not often, employed. The author says that thin sections of the "killas from Polgooth Mine," when examined by transmitted light under a low power, exhibit no decided evidence of structure, but appear as a milky-white mass in which are disseminated numerous moss-like semi-crystalline markings of a brownish-green colour. They are also observed to be traversed by various fissures, which have become filled by crystalline transparent quartz. When examined by a higher power this rock is found to consist of an aggregate of minute granules intimately blended together, and without definite outlines. There were also some grains of oxide of iron and other dark markings not sufficiently opaque for this substance, some are apparently hornblende, and others are probably patches of a chloride mineral. This slate has undergone so great a change that the original grains of which it is composed can only be made out with the polariscope. Speaking of killas from Polmear Mine, 40 fathoms below the surface, the author having first given the chemical composition, says that sections prepared from this rock do not differ materially from those obtained from the Polgooth specimens. The ultimate fragments of which it is composed are exceedingly minute; but granules of quartz of an appreciable size, and affording colours with polarized light, are disseminated throughout the fine-grained matrix. A greenish tint is also imparted to it by chlorite; and it is minutely divided by a system of markings made up of pairs of nearly parallel lines, each about $\frac{1}{1000}$th of an inch in length and $\frac{1}{6000}$th of an inch apart. These cross each other so thickly as to form a kind of close network, and give no colours by polarized light; they were at first taken for amorphozoa; but his friend Mr. R. Etheridge, who kindly examined them for him, is of opinion that they are "certainly not organic." In regard to the "Roofing-slate, Delabole," the author, unable to arrive at a decisive opinion, sent a specimen to Mr. Sorby, F.R.S., who says:—"The dark crystals in the slate from Delabole are similar to what I have seen in many others. As far as I can make out, they are imperfect hexagonal plates; and since the general character clearly shows that they must at all events contain much iron, it seems very probable that they may be more or less altered specular iron. This, of course, would well agree with the fact of its containing titanic acid." The author gives several other examples; but for further information we must refer our readers to the journal in which the paper originally appeared.

Species of Atax Parasitic on Fresh-water Mussels.—M. Emil Bessels has written a paper on the above subject in the 'Würtembergische Naturwissenschaftliche Jahrrshefte' for 1867, which has been translated by Mr. Dallas into the 'Annals of Natural History' for January. The paper is of some little length, and will well repay perusal. His observations have been especially made on the development of *Atax ypsilophorus*, and as far as originality is concerned refer to the formation of the blastoderm. How long, for instance, after the deposition of the eggs the blastoderm makes its appearance no one can say with certainty, inasmuch as the deposition itself cannot be observed. In eggs which were taken from the branchiæ of the *Unio* or *Anodonta*, and apparently had undergone no change after deposition, he usually detected the first traces of the blastoderm in from two to three days. It is formed insularly, as may be easily proved by opening an egg carefully in a solution of 1 per cent. of bichromate of potash. It is impossible to ascertain the process of formation by the direct observation of the uninjured egg, on account of the dark colour of the yelk. After the blastoderm has grown round the whole of the yelk, the embryonal envelope which Claparède describes as the *deutovum* separates from it. This is produced in exactly the same manner as the larval membrane of the Crustacea, as observed by Van Beneden and myself in various species of *Gammarus*.* Claparède was at first inclined to regard this envelope as the homologue of the structure which in insects has received the unfortunate name of the "amnion;" but he soon gave up this comparison. M. Bessels, on the other hand, regarded the membrane in question in the Mites as homologous with the larval membrane of the Crustacea, and the latter as homologous with the "insect-amnion," for which he has elsewhere proposed the better name of "protoderm." Shortly after the formation of the embryonal envelope, we see, between it and the blastoderm, the first amœboid cells (*hæmamœbæ* of Claparède). He remarked that these cells "are blood corpuscles of quite abnormal derivation." In using this expression he had the circumstance in his mind that they are formed from separated blastodermic cells, which, at the time of their production, are the sole cellular structures that are found in the egg. He did not then feel it necessary to say anything more upon this point, as the publication of his original memoir was to be expected. He thought at first that the blood corpuscles were all developed from separated blastodermic cells, and only afterwards, perhaps after the formation of the buccal orifice, passed through this into the embryo. As, however, he never saw any such migration of the cells, even after observing them for hours, he has given up this view, and now thinks that there is a further formative focus for them in the interior of the embryo. His present opinion as to the hæmamœbæ is, that they really agree perfectly in form and behaviour with blood corpuscles, but nevertheless cannot be regarded as blood corpuscles. He sees in them appurtenances of the embryonal envelope which Claparède denominates the *deutovum.* Whilst at the commencement of embryonal development of many insects a *cellular* envelope separates from the blastoderm, and in some crustacea a larval skin, which is usually *structureless*, in *Atax* a larviform structure first separates from the blastoderm, and shortly afterwards the contractile cells. This state of things, when regarded in this manner, furnishes an additional reason for regarding the embryonal envelope of *Atax* as the homologue of the protoderm of insects.

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* E. van Beneden and E. Bessels, "Résumé d'un Mémoire sur le Mode de Formation du Blastoderme dans quelques groups de Crustacés," 'Bull. Acad. Roy. Belg.,' 2e sér., xxv., p. 443.

Observations of Pachytragous Sponges.—Some observations of an interesting character have recently been made by Mr. H. J. Carter, F.R.S., on certain sponges from the south coast of Devon. The chief point of interest in these sponges—*Dercitus niger*, *Stelletta aspera*, and *S. lactea*—is the presence of peculiar cells in *Dercitus niger* and *Stelletta aspera*, corresponding in multiplicity, position, and general distribution, though not in composition, to the globular crystalloids or little silicious balls in the crust and body of the Geodidæ; add to this their contents, which render them so much like reproductive agents, and, lastly, their occurrence in the two sponges mentioned, and not at all in the third, *viz. Stelletta lactea.* Nor do they exist in *Pachymatisma Johnstonia ;* but in the dried specimens of *Geodia gigas*, presented to the British Museum by Dr. Oscar Schmidt, there are similar cells in abundance, together with the globular crystalloids. Although analogous in multiplicity, position, and distribution to the globular crystalloids in the Geodidæ, they not only differ from them, as just stated, in composition, by the former being cellular and albuminous, while the latter are solid and silicious throughout,* but also in size; for the largest crystalloids are three or four times as large as the largest cells, and the latter much larger than the smallest or youngest crystalloids, so that in these respects, *viz.* in composition and size, they cannot be confounded. Formerly Mr. Carter thought that the colour of the sponges might be always sought for in the ampullaceous sacs ("Wimperkörbe," Schdt.), and therefore that the black cells of *Dercitus niger* might be ampullaceous sacs;* but the result of more particular examination subsequently, as given above, has caused him now to regard the latter more as reproductive agents. He has also alluded to the presence of ampullaceous sacs in *Geodia gigas*, Schdt.; but on examining these also again, now that he has become more intimately acquainted with the composition of the cells in *Dercitus niger*, &c., he is led to conjecture that they also may be of the same kind as the latter, in which case, should he be right, we shall have an instance in this sponge where both the globular crystalloids and the cells occur together, and thence have to seek for the ampullaceous sac under some other form than that in *Halichondria simulans*, not only in *Geodia gigas*, but in *Pachymatisma Johnstonia* and in *Stelletta lactea*, &c., where there is nothing of the kind like the ampullaceous sac of the *Halichondria* mentioned, so far as the larger size of its cellules and peculiar grouping go. The ampullaceous sac with smaller and thus less-marked cellules may exist in all; but as yet he has not been able to substantiate this. Of course, after having been dried, it is impossible to make out anything in these cells so satisfactorily as in living ones; and hence, although such cells are present in great abundance in their contracted state in the dried specimens of *Geodia gigas* mentioned (measuring about 1000th of an inch in diameter and filled with a number of cellules), liquor potassæ, although it causes the cellules to run together into one homogeneous mass, does not yield any satisfactory demonstration of a nucleus under the addition of nitric acid, nor is the cell-wall well marked—two points in which the cell of *Dercitus niger* differs distinctly from the ampullaceous sac.

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* 'Annals of Natural History,' 1869, vol. iv., p. 16, &c., pls. 1 and 2, figs. 12 and 14.

Cancer of the Kidneys in Children.—Dr. P. M. Braidwood, who has contributed an excellent article on this point to the 'Liverpool Medical and Surgical Reports' (vol. iv.), gives the following as his experience of the structural degeneration seen in the gland:—The firm yellowish, and the whitish less consistent portions of a cancerous kidney are found to be composed of delicate fibres enclosing in their meshes fusiform, round, oval, or irregular cells. The fibres are observed to be extremely delicate, distributed sparsely, and enclosing cancer-cells, which are small, oval, or round. Among the fluid contents of the cysts are seen large, oval, round, or irregularly-shaped, multinuclear cancer-cells. Generally at one part in the circumference of such a cancerous mass is to be seen a reddish edge of seemingly normal tissue, which on microscopical examination is discovered to consist of tubuli uriniferi and their malpighian terminations undergoing cancerous degeneration. The urinary tubules at such a point appear to be lined by very minute, round cancer-cells, which, on being detached, or when submitted to the action of dilute acetic acid, exhibit their multinuclear character and granular contents. At certain turns in the tubules the cancer-cells are accumulated in heaps, while in the malpighian corpuscles they are large, subdivided, and contain numerous nuclei. It seems probable, then, that the connective tissue cells of the cortical and inter-pyramidal portions, and likewise the epithelial cells of the proper renal tissue, are transformed into or replaced by cancer-cells.

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* 'Annals,' 1870, vol. vi., p. 332.

A New Genus of Graptolites.—Mr. J. Hopkinson, F.R.M.S., F.G.S., who gives a very interesting paper on *Dicellograpsus* in the 'Geological Magazine' (vol. viii., No. 1), has used the microscope in his investigations. The author's remarks, which extend to the description of five new species, are too long for abstract; but the paper is full of interesting facts, which show how valuable the microscope is in geology.

CORRESPONDENCE.

The Errors of Lenses—Mr. Wenham and Dr. Pigott.

To the Editor of the 'Monthly Microscopical Journal.'

Ballinamallard Rectory,
Co. Fermanagh, *Feb.* 15, 1871.

Sir,—A well-known philosopher of former times, Scaliger, used to teach that for getting at the truth in a difficulty there was nothing like controversy; "for as light," he says, "from the collision of flints, so truth is struck out from the collision of minds." Whether this be so or not in general, in the present controversy in your pages it seems in some danger of failing; for in the last paper of Dr. Pigott the point at issue had been so obscured by things not relevant, that, except to very watchful readers, it may seem doubtful whether anything at all has really resulted from the discussion. As, however, the question has in fact been reduced to a very narrow and a very definite issue, it would be a thing to be regretted if this result should be lost sight of. I would, therefore, with your permission, point out how it now stands.

Immersion lenses being admitted to be superior to the others, Dr. Pigott claims to have discovered the cause of this superiority, the object supposed mounted in a medium like balsam. His theory is that they have a greater aperture. For with the other object-glasses the more oblique rays are lost by total reflexion at the cover; but with the water-lens a much wider pencil is transmitted to the eye. And in token of this he constructs the elaborate diagram, Plate LX., to show how much exactly is the difference in the angle. By this he claims to have cleared up "the mystery of the water-lens."

To the first assertion—*viz.* that with the common lens the angle cannot exceed 82°—Mr. Wenham replies, this is true; but it is not new, having been pointed out long ago by others, himself among the number.

To the second part—that the more oblique rays can enter with the water—he replies, true they can enter the *front* lens; but to what purpose? From this lens they emerge too oblique to enter the back combinations, and therefore they do not reach the eye. If the reader will turn back to the upper of the two figures in Plate LX., this will be very plain. Take one of the more oblique rays, *e.g.* that numbered 6 or 7, and produce it straight on (there is no change here by refraction), then it is at once seen that such a ray will pass outside the back combinations and be lost.

This is, as we might say, checkmate,—in one move. Dr. Pigott committed the oversight of forgetting that the back lenses must be taken account of. Having seen his rays safely into the front glass, he takes no thought for the rest of their journey, assuming that if they can enter the front they will also enter the eye.

In strictness, no more is required so far as Dr. Pigott's discovery is concerned. His theory is shown to fail in its proof, and this quite independently of anything that may be put in its place. Mr. Wenham, however, goes farther than this. He supplies a correct figure, with the rays projected through a real object-glass, to take the place of Dr. Pigott's imaginary one; and he points out how to determine precisely the limit of the aperture which the immersion lens does admit of. The reasoning by which he fixes this, which is somewhat close and concentrated in form, is given in the second paragraph of p. 17, and may require some slight attention from the reader to follow it. It may be paraphrased as follows:—Taking a dry object-glass, the pencil issuing from the object cannot exceed 180°. This being refracted by the flat surface, is converged to a pencil, which cannot exceed 82°. This is again refracted at the second surface of the front to a narrower pencil, whose angle determines the figure of the back lenses and the pencil *they* admit. Remove now the dry front, and substitute the immersion, other things the same. To find the extreme ray of our new objective, we must begin to trace its course in the place where it is known; that is, we trace it inversely from the back combinations, for which we have already fixed the *maximum* limit. This we have found to be such that, tracing inversely to the inside of the front, the rays will there be found converging at an angle not exceeding 82°. These we trace thence into the balsam (without change) till they converge at the focus, thus determining a *maximum* aperture of 82°, the same precisely as for the dry lens.

Unfortunately, Dr. Pigott has omitted to read this, or else has failed to understand it. That *something* is wrong in his diagram he has indeed become conscious after seeing Mr. Wenham's figures; and in a foot-note to p. 70 he admits that his own diagram will not work, but curiously he regards this as of no consequence. He accounts for the mistake, and expects it should be allowed to pass on the very remarkable plea that when he made it he was thinking only of the *front*. Precisely so; his mistake was that he thought only of the front when he *ought* to have thought of the rest as well. The aperture of an object-glass is measured not by what reaches the front but by what reaches the eye.

Having in this way disposed of his own mistake, he points out one of Mr. Wenham's, which he fancies he has discovered—a mistake which seems to him so odd that it affords him a good deal of amusement, and is in his opinion a legitimate subject for "chaff." In determining the immersion aperture, it seems that Mr. Wenham immerses the front of the glass not in water but in balsam; thus, as his critic pleasantly says, "demonstrating the principle of the water-lens by applying it *without* the water to prove its action *with* it (save the mark!)." Here Dr. Pigott is amused only because he does not understand. In the water-lens there is glass, water, then glass again, the last in contact with balsam. Mr. Wenham, when tracing backwards the course of the ray, to avoid complications not essential, treats the water as if it were a continuation of the glass, properly neglecting the effect of the water on the obliquity because it *has* no effect, since the ray after leaving the water film recovers its former direction.

The other parts of Dr. Pigott's paper do not touch the question. The aquariums, the gold fish, Claudius Ptolemy, the bent oar, and the laws of refraction, are nothing to the purpose, though brought in as if they were decisive. It is for this reason I have said that the question was obscured by the introduction of things irrelevant. The history of Claudius Ptolemy is no more to the point than the history of Claudius Cæsar or Claudius Lysias. No more are the laws of refraction and reflexion, repeated (for about the twentieth time,) because these laws are "first truths" and are known to everyone, and never are nor ever have been denied by anyone great or small. There is, no doubt, a sense in which this or any other optical question may be said to depend upon them; the same sense in which we may say that a question about the nutation of the earth's axis or the motion of the moon's apse depends upon the multiplication table.

These laws are brought in on every occasion with much arithmetic, and mathematics done with Greek. This display of learning seems to have impressed his readers so much that even skilful observers fear to trust their own faculties against mathematics so original and profound. In a former letter I pointed out that those who looked into it might find there was some slight misapprehension about this. No doubt it is very hard for non-mathematical microscopists to think so,—to believe that so much mysterious mathematics can have nothing in it, especially after their nerves have been shaken by meeting such words as *Pneumo-spherical* and *Hydro-spherical*, *Aberrameters*, *Kratometers*, *Refractometers*, *Eidola*, and all the rest. But so it is. The whole of this "original" learning from beginning to end, the picture of the bent oar not excepted, is simply copied out of the first pages of the little books for beginners, the primers and horn-books of optics. Examples are given in the primers to be worked out, and Dr. Pigott works out other examples with the numbers changed,—that is all. The "original" part is that he works them with infinitely more trouble than is needed;* which explains his statement that they have cost him, as evidently they have, "immense labour." What is not so easy to explain is his not having discovered that what costs him so much trouble, costs others neither trouble nor time. . And when they are put forth month after month as "original researches," we can do nothing but look on in silent wonder.

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* As an example typical of this superfluous work, we may take the case of the mercury globule in pp. 265, 266 (vol. iv.). Beginning, as he does, with the primary

Your obedient servant,

S. Leslie Brakey.*

A Proposal as to Diatoms.

To the Editor of the 'Monthly Microscopical Journal.'

Dear Sir,—I think the thanks of all amateur microscopists who are interested in making classified collections of diatoms are decidedly due to Captain Lang, the President of the Reading Microscopical Society, for his generosity in making public his method of selecting and mounting diatoms, and for the very interesting paper on the subject which appeared in the December number of your excellent Journal. It is as a rider to his paper that I venture to trouble you with these few lines, in the hope you will publish them, as I am of opinion that if by a discussion of the subject some mode could be discovered or agreed on by which amateurs might be able to purchase prepared or unprepared diatomaceous material, or to interchange small quantities—say even a single dip evaporated on a glass slide—of gatherings containing dissimilar forms, great benefit would accrue therefrom. I have had the pleasure of conversing and corresponding with Captain Lang on these points, and I know that his ideas and mine on the matter are very similar; therefore I have the less hesitation in asking you to ventilate the subject.

At present, among amateurs, there exists much difficulty in getting material which shall contain a great variety of species and

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equation, writing it in numbers instead of symbols we get *at sight*, without calculation, the value of q, and the required diameter is, *at sight*, one-fifth of it. This is the whole work. Not a cipher nor a letter has to be added. The reader can now turn to p. 266, and, if he likes, improve himself in algebraic fractions, the Binomial Theorem, and Infinite Series, by working through Dr. Pigott's "calculation." As for the equation itself, this and the method to be followed, and the figure, everything except the wonderful workmanship, are copied out of the manuals,—a circumstance which Dr. Pigott did not think it necessary to mention.

* *P.S.*—Since this was written some prominence has been given in the last Number to the instrument introduced, or revised, under the name of the "Aplanatic Searcher." I ought, therefore, perhaps to add that none of the above remarks have any reference to it: as its principle or construction has not formed the subject of any one of the papers contributed by Dr. Pigott to this Journal. Time and the sure test of experiment will determine its practical value; meantime those outside a certain circle, knowing nothing, have of course nothing to say. My criticisms apply only to the Mathematical Optics in this series of papers, about the merit of which there has grown up, as it seems to me, so very remarkable a misconception; remarkable as showing the incredibly low level at which the *scientific* knowledge of optics exists among English observers, when many of them—so skilful and accurate in observation as every part of this Journal proves them to be—can yet believe these papers to be an accession to the stock of our knowledge.—*March 8th.*

genera. You may make gathering after gathering in your own neighbourhood, but though you may make a large collection of material, which you could not use up in your lifetime, your gatherings would very likely contain but multitudinous repetitions of the same class of diatoms. It would probably be the same with the gatherings you might make in your sea-side trips, and still more probably so in your preparations of guanos from different localities. The consequence is that you would in all likelihood get tired of making gatherings, and having the trouble of preparing and cleaning them.

Diatomaceous material, either prepared or unprepared, cannot, as far as I am aware, be bought unmounted, whether it be of British or foreign genera; and the present mode of exchanges amongst amateurs, carried out through the exchange columns of different journals, is unsatisfactory in the extreme, as one can never know what kind of rubbish he may have sent him in the way of barter.

The professional preparers will not sell material in even the smallest quantities. It seems almost incredible, but let the following statement, which I vouch for as fact, speak for itself, and you will see, that though to prepare a mixed slide for the market the preparer has to put a certain sized dip of material on a glass slide and evaporate it, if you ask him to stop the process there, and sell you that slide *for a shilling*—the same price, mind you, that he will ask for it when it is completely mounted and ready for the market—he will not sell it you. No! He will cover it with Canada balsam and a glass, he will clean off the superfluous material, and perhaps take the trouble of putting on a ring of cement, and when he has thus spoilt it for the purposes for which you want it, he will offer it you for a shilling. This is precisely what happened to me. I offered to a well-known preparer of microscopic objects, whose name I will not mention, a shilling a-piece for a single dip—the ordinary quantity put on a slide —each of certain deposits from which I wanted to select forms myself for my own cabinet, and he refused to let me have them. I thought I should succeed in my object by ordering some of these preparations *mounted dry*, and by melting the cement and removing the cover I could get what I wanted. But do you think I was allowed to have them? No! It is usual to mount them in balsam, and in balsam I must take them, or go without. There is a certain class of diatoms only, that the mounters put up dry for the market, and from these only are you allowed to select. Foiled in my first attempt, I made application to another equally well-known mounter, and I should like to publish his name, because he did at any rate write me a very civil letter in reply, showing me that it was not his fault that he could not comply with my request, but as I have not his authority to do so, and he might not like it, I refrain from doing so; but I give the text of his letter, as I think it may be interesting. He says:—

"Sir,—I beg to assure you that I have not forgotten or neglected to notice your letter of the 14th instant, but have given to it frequent thought. I regret that I cannot comply with your wish, from various causes. Many of those diatoms are not my own mounting, and those who have the material would not listen to your wishes, I am sure, however reasonable they may seem. If I were to try, it would take me immense labour to write a long letter to each of twenty persons, perhaps, which ten out of twenty would not understand, and would not reply to; and the few who might comply would be almost sure to send something not pleasing to you. If you go one inch out of the daily beaten track every obstacle is thrown in the way, and everything that is possible is done wrong. If I could get you twenty dips the *time* alone would be worth 20*s*. "With thanks, I am, &c."

Now, I should think something might be done to make these vendors more reasonable, who thus attempt to force amateurs who want to make a systematic collection of selected slides, to purchase only their selections, at from eighteenpence upwards; or if they cannot be brought to reason, we have the remedy in our own hands if we choose to apply it. In London, or in such a town as Liverpool, where ships are daily coming in from all parts of the world, I am sure it would pay an optician, as a business speculation, to order a quantity of diatomaceous deposits and foraminiferous sands, &c., &c. The localities where they are to be obtained are perfectly well known; the price of the raw material in those places is little more than the cost of labour in collecting them. The freight of a few hundredweights would be trifling, and they could be sold, either prepared or unprepared, at two or three hundred per cent. profit, and if properly advertised, and if notification of the fact of their being on sale were sent to all the Secretaries of Microscopical Societies in England, I venture to say the deposits would not remain long on hand.

It may be said that it is difficult, if not impossible, to find out what towns possess Microscopical Societies. I think this difficulty might be met by proposing that any provincial association might be affiliated to the Royal Microscopical Society by paying a yearly fee of, say, half-a-crown to have its title and the names of its President and Secretary registered; and if lists of such societies were periodically issued, sufficient publicity would be obtained.

With regard to British species, nothing could be easier than to facilitate their dissemination by the following plan. It is astonishing what a very small quantity from any gathering is amply sufficient for a man's own use. Suppose that every member of a Microscopical Society who made a gathering or preparation, whether marine or fresh-water, after carefully examining it, and noting the particular diatoms it contained, should present what he did not require for his own use to the society to which he belonged, of all of which preparations the Secretary should make a list. Now, although the individual might not like to sell his preparation, I can see no reason why the society might not sell it, at so much a dip, according to its plentifulness or scarcity, and the trouble and expense of cleaning it; but I should think from 2*d*. to 6*d*. a dip would be ample, and a common medium for advertising these could easily be found. Such a mode of proceeding would, of course, not interfere with gentlemen exchanging their mounted slides as they do at present.

Can you or any of your readers suggest a better mode of obtaining material, by which so many more of us might be able to profit by

Captain Lang's happy discovery? If not, can you assist in carrying this out?

It is strange that microscopists should not generally show a greater readiness to assist each other than we usually find. I met with a singular case of want of courtesy a short time ago. The President of a Microscopical Society having informed me that fine specimens of foreign diatoms were to be found among the Californian shell-washings, which shells were bought by the dealers in *Liverpool*, who sold the washings, but that he could not give me the address of a dealer, I wrote to the Secretary of the Liverpool Microscopical Society, asking for the information, and enclosing a stamped envelope for reply. My letter never came back to me, so I presume it was delivered; but I have never been favoured with a reply. If any of your readers could furnish the information, in a letter addressed to Captain Knight, Claremont Villa, Leamington, I would gladly pay the postage.

Yours faithfully,
R. D. KNIGHT.

A FRAUDULENT COLLECTION OF SUBSCRIPTIONS.

To the Editor of the 'Monthly Microscopical Journal.'

ROYAL MICROSCOPICAL SOCIETY, KING'S COLLEGE, *March* 20.

DEAR SIR,—It has just come to my knowledge that some one has been sending out the collecting cards of our late collector, Charles Low, and is endeavouring to obtain subscriptions by this means.

Will you allow me to caution and inform our Fellows that no one is authorized to collect subscriptions for this Society besides the Treasurer and myself, and that receipts are not valid unless signed by either of us.

I am, Sir, yours truly,
WALTER W. REEVES,
Assistant-Secretary.

ERRATA: THE FIGURES OF CRYSTALS WITH SILICA.

To the Editor of the 'Monthly Microscopical Journal.'

ASHDOWN COTTAGE, FOREST ROW, SUSSEX,
March 6, 1871.

SIR,—An unlucky accident delayed my receiving and returning proofs of Plates LXXVII. and LXXVIII. in March number of the Journal, and led to the omission of correct numbers referring to the paper.

If your readers will begin with Plate LXXVIII., and number the figures in horizontal lines from 1 to 6, and in Plate LXXVII. from 7 to 13, they will find descriptions and figures coincide.

I am, &c.,
HENRY J. SLACK.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *March* 1, 1871.

James Glaisher, Esq., F.R.S., took the chair.

The minutes of the last meeting were read and confirmed.

The Secretary announced that in consequence of certain losses which the Society had had the misfortune to sustain through the illness of the Collector, and of the fact that there was a considerable amount of subscriptions in arrear, the Council had felt themselves compelled to advise that the usual soirée and evening party should not be held this year. Many Fellows of the Society had expressed the wish that in addition to their ordinary meetings there should be a social gathering of a scientific character, where opportunities for examining objects and discussing points of interest would be afforded. The Council therefore would advise that the Fellows should consent to forego the expensive evening party for the current year, and that some convenient evening in the month of April (say the latter part—about the 26th) should be fixed upon for holding, not a soirée, but a scientific meeting, limited to the Fellows and a few scientific visitors. The Council wished to take the opinion of the meeting on the question of providing refreshment during this meeting. If the Fellows should decide not to incur that expense (say 7*l.*), the amount thus saved could be devoted to the purpose of defraying the expense of another scientific meeting later in the year.

The Chairman in putting the proposition with regard to refreshment to the meeting, said that the proposal which had been made was to be considered not in the light of a precedent, but merely as relating to arrangements for the present year. The vote of the Fellows was then taken on the question of refreshment, and it was decided by a large majority that none should be provided.

Mr. Wm. Kitchen Parker, F.R.S., F.Z.S., the newly-elected President, who had been detained by a professional engagement, appeared at the table at this period of the proceedings, and was installed into the Presidential Chair amidst the cheers of the meeting.

A list of donations was then read, and the thanks of the meeting given to the respective donors.

Mr. Jabez Hogg (Secretary) stated that in addition to the list already read he had received a small series of photographs from Mr. Hennah, of Brighton, showing some illusory appearances which have occupied the attention of the Fellows lately in regard to the beadings on the Podura scale. By a simple arrangement of small cylinders of glass placed at right angles to each other and illuminated in different directions, as they revolved curious and illusory appearances might be obtained. The illusion is due to the glass cylinders virtually becoming lenses when crossed, and is still further enhanced by the focus of objects behind being within or upon the surface of the rods. Mr. Hennah would not be understood to maintain that because in these photographs he had obtained *pseudo*-beads no beaded structures existed. He believed in some cases they might exist, although in the majority he was of opinion that they were phantoms of refraction and reflexion arising from special illumination of transparent structures by transmitted light. The photographs must fulfil a mission of caution, and he (Mr. Hennah) hoped would lead to more careful examination of debatable things.

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* Secretaries of Societies will greatly oblige us by writing their reports legibly—especially by printing the technical terms thus: Hydra—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. M. M. J.

The effects photographed were obtained in the following manner:

1. Sheet of ground glass close to rods; illuminated a little obliquely.
2. Sheet of ground glass 16 inches square about 1 foot behind rods; slightly oblique illumination.
3. Same as 2, but with a 6-inch paper cross on ground glass, about 6 inches from rods.
4. Common "knobbed" water-bottle close to rods.
5. Rods obliquely; a window with curtains.
6. Rods more oblique than 5; an open window quite in focus.
7. Rods same as last; same arrangement, but out of focus.
8. A photograph behind two rods; placed at right angles, showing distortion.

It was announced that as there were more papers in the hands of the Secretaries than could be read during the evening, the following would be taken as read.

A communication from the Rev. Professor Gagliardi, "On Views of *Surirella gemma*," and another by Mr. Cubitt, "On the Winter Habits of Rotatoria," would be taken as read.

In alluding to a paper contributed by Lieut.-Col. Dr. Woodward, of the U. S. Army, the Secretary called attention to a series of beautiful photographs of the Podura and other test-objects which Dr. Woodward had presented to the Society.

Mr. J. Bell then read a paper "On the Microscopic Examination of Water for Domestic Use."

Dr. Lawson inquired under what powers Mr. Bell had made his examination.

Mr. Bell said he had used a $\frac{1}{4}$th object-glass and B eye-piece, and a $\frac{1}{16}$th and B eye-piece.

Mr. Slack attached great value to Mr. Bell's researches. He thought that it would require a very large body of experiments before it would be safe to infer the composition of a solution from the presence of particular forms of fungi. In comparing some specimens of fungi given to him by Mr. Greenish in solutions of arsenic and strychnia, with some in silica which he (Mr. Slack) had in dialysed solutions in distilled water, he thought that though the appearances differed to some extent, the plants were of the same kind. As regarded filtering through sand, part of the action was chemical and part mechanical. Passing water through a mass of sand was a very good mode of oxidizing, as well as of filtering. How far chemical action operated in the case of Mr. Bell's experiments it would be impossible to say without further investigation. With regard to the different sorts of fermentations, he thought it would be found that more than one fungus contributed to vinous fermentation, and when vinous passed to acetous fermentation it would be found due to the action of other sets of fungi, and the same with butyric fermentation. If a piece of vinegar-plant be taken and separated into successive sheets, and dried in the oven, it would be reduced very much to the appearance of prepared gelatine, and will even "chink" when thrown down on a plate. This dried vinegar-plant would often excite butyric fermentation, and he noticed a slight odour of butyric acid in one of Mr. Bell's bottles.

Dr. Millar thought that Mr. Bell had established one remarkable fact, namely, that old filters were better than new ones.

The thanks of the meeting were presented to Mr. Bell.

Mr. Slack then read a communication from Lieut.-Col. Dr. Woodward "On the Structure of the Podura Scale, and certain other Test-objects, and of their Representation by Photo-micrography."

Dr. Pigott said he should be very glad if this vexed question of the Podura markings could be agreed upon in some way; it had been such a valuable scale in the construction of object-glasses that it could not be prized too much. He would say, in reference to Dr. Woodward's remarks, that before anyone depended upon the photograph of an object as indicating real structure, they should be intimately acquainted with its various aspects under the microscope, and decide which could be relied upon. In photographing any object, the operator should remember that the photographic lines show the actinic rays which appear at the moment, and if the photographs were to give unbiassed evidence they ought to be made under every possible condition of focussing and correction. Dr. Woodward does not appear to have borne this sufficiently in mind. He was, nevertheless, extremely obliged to him for his paper, for, having placed himself at the head of photographic science in reference to the microscope, it was satisfactory to know that he had brought his great practical power to bear on the solution of this subject.

The thanks of the meeting were given to Dr. Woodward.

The President then said: The honour which the Society had conferred upon him by their election of him to the chair was quite unexpected. He was not a professed microscopist, but a microscopic observer, and one who felt very thankful to the ingenious and talented men who from time to time had made improvements in the instrument. He thanked the Fellows for his election, and should take great pleasure in rendering the Society any service that lay in his power. His first visit to a scientific society was made to the Microscopical, eighteen years ago, and for years he had attended its meetings until allured therefrom chiefly by his devotion to the study of a gigantic form of boatbill (*Balaniceps*) which had been brought over to this country. He had also worked largely at the Foraminifera. He had intended that evening, if time had permitted, to read a short paper illustrating the use of the microscope in developing the embryology of the skull. His work had not direct reference to the subject of teleology, nor to the structure of the tissues; but his object had been to work out the metamorphosis of the skull, and to see the tissues as they begin to differentiate and modify to form the embryo. The subject was a very large one, and had been principally laboured at by the great German embryologists. He had spent the last two years in studying the development of the frog's skull, in watching the different and numerous stages which that creature undergoes, and the relations it bears to other creatures of the vertebrate type, always remembering that the frog was essentially a fish. He had been in some degree unprepared for the extent of the metamorphic changes that the frog underwent. He had worked out this subject into ten artificial stages, the first of which he had obtained when the frog was in the egg. In the first stage of its morphological development the animal was two stages below the youngest *described* embryo of the lowest kind of fish, but one. The larva of the lamprey was the earliest condition of a fish's skull with which we are acquainted. He had succeeded in getting two stages below the larva of the lamprey. From this stage he had worked up the development of the frog until he came to the tadpole, which is the representation of the types of rays and sharks. As he ascended in the various stages the likeness to the other vertebrata became very apparent. In an adult frog (*Rana temporaria*) he had obtained a metamorphic development of such height that it bordered upon our own class, the Mammalia. At the same time it should be stated that other parts of that frog's skull retained the simplicity of the adult lamprey. In the frog we had a creature who had run across the whole circle of types, creeping gradually up to the Mammalia, and yet never losing his relation to the original type, but retaining its structure and relation to the very end, although subdividing and metamorphosing certain of the facial arches into the very number of parts that we have in our own inner ear. The chain of bones in the human ear (the hammer, the anvil, the round bone, and the stirrup) had caused a great deal of trouble to anatomists in their attempts to trace the series of metamorphic changes. He had, however, made this clear by tracing the history of the facial bones of a frog, a creature which was but a fish in respect of its earliest embryonic conditions. Supposing the doctrine of development to be true, it would seem that we ourselves have come originally in some line sub-parallel to the frog (he would not say from the frog itself, although man had repeated the form tail-less). Even in the highest oviparous vertebrates no subdivision of a facial bar to form that tiny but really important part of the human skull, the *os orbiculare*, ever obtained. In this respect the frog comes nearer to the Mammalia than any bird. Birds have branched out in a direction quite away from the ordinary line, and have culminated in their own glorious types. If it is desired to trace the development of the Mammalia, inquiries must commence with the Batrachia; and in such inquiries the thought constantly occurs that between us and the Batrachia there have been lost whole groups of creatures. We were only just beginning to see the manner in which the work of tracing the development of the higher forms of animal life was to be carried on. He hoped at the next meeting to bring a paper on this subject.

Mr. Ahrens exhibited a new binocular microscope of ingenious construction, which could be worked with the highest powers.

Dr. Pigott gave a brief description of an instrument for increasing (?) angular refraction.

Mr. Richards exhibited a cheap form of pocket microscope.

The President will read a paper "On the Mode of working out the Morphology of the Skull" at the next meeting, April the 6th, to which day the meeting was adjourned.

OFFICERS AND COUNCIL OF THE ROYAL MICROSCOPICAL SOCIETY, ELECTED 8TH FEBRUARY, 1871.

President.—*William Kitchen Parker, F.R.S.

Vice-Presidents.—Charles Brooke, M.A., F.R.S.; *John Edward Gray, Ph.D., F.R.S., F.L.S., &c.; *John Millar, L.R.C.P.Ed., F.L.S.; Francis Herbert Wenham, C.E.

Treasurer.—Richard Mestayer, F.L.S.

Secretaries.—Henry J. Slack, F.G.S.; Jabez Hogg, F.L.S.

Council.—Robert Braithwaite, M.D., F.L.S.; *John Berney, Esq.; *James Glaisher, F.R.S.; *William John Gray, M.D.; Henry Lawson, M.D.; Henry Lee, F.L.S., F.G.S.; James Murie, M.D., F.L.S.; *G. W. Royston-Pigott, M.A., M.D. Cantab., &c.; John Ware Stephenson, F.R.A.S.; Charles Stewart, M.R.C.S., F.L.S.; Charles Tyler, F.L.S., F.G.S.; *Thomas Charters White, M.R.C.S.

Donations to the Library from February 8 to March 1, 1871:—

	From
Land and Water. Weekly	*Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*W. W. R.*
Quarterly Journal of the Geological Society, No. 105	*Society.*
Journal of the Quekett Club	*Club.*
On the Claims of Science to Public Recognition and Support. By Dr. Guy, F.R.S.	*Author.*
Journal of the Linnean Society, No. 50	*Society.*
A set of photographs. By Mr. Hennah	*Mr. Hennah.*
A set of photographs. By Dr. Woodward	*Dr. Woodward.*

John Ligertwood, M.A., M.D., was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

January 26th, 1871.—Microscopical Meeting. Mr. T. H. Hennah, Vice-President, in the chair.

Mr. R. Glaisyer announced the receipt of three slides from Mr. Wonfor for the cabinet.

Mr. Sewell, who had proposed the subject for the evening,—the use of the Polariscope in the determination of Structure,—remarked

* Those with the asterisk placed before their names are newly elected.

that with most microscopists the polariscope was merely a toy to show pretty objects, whereas he believed it ought to be applied more than it was to the making out of structure. Having inquired of Mr. Curties respecting Hislop's selenite stage, he was informed that Mr. Ackland had contrived a very simple one, which he thought he could borrow; but he had done better, for he had induced Mr. Ackland to come down to exhibit and explain his selenite stage.

Mr. Wonfor considered a large number of microscopists used the polariscope as a scientific instrument in making out structures.

Mr. Ackland was sure the majority used it as a toy, he might say fifteen out of twenty. Scarcely any had a selenite plate with the axis marked. He did not think once in twenty years had he been asked to mark the axis.

Mr. Wonfor thought that a large amount of work was done by microscopists without using the selenite at all, and then only with objects possessing slight depolarizing power. They would all feel obliged by Mr. Ackland's pointing out the advantages of marking the axis.

Mr. Ackland said, if the axis of the selenite film was marked, it was possible to determine the tension of any object, especially, say, in the examination of muscular fibre, did it point out the direction of the tension. The Germans, who were greatly in advance of us in minute anatomy, always used marked selenites. One of the only persons he knew using one in London, was Mr. Stewart, of St. Bartholomew's Hospital. Selenite films, giving the various tints, blue, green, yellow, red and purple, were commonly used; but all must have noticed, when examining an object which did not fill the field of view, that the colour of the background did not harmonize with the colours of the object. It occurred to him that a *neutral* tint, corresponding to the tint occurring in Newton's rings, midway between the violet of the second wave and the indigo of the third wave, was the one required. In a parcel of 500 films he only found one giving this tint; using it with an object he was delighted with the effect; happening to put a duplicate object, which, with an ordinary film, gave exactly the same colours, he was struck with its comparative poorness. Trying the first slide again, he obtained the same good result. This difference, he thought, was either owing to the thin glass cover being at the right axis for the colour, or the glycerine in which it was mounted exerting an influence. As the neutral films were so difficult to obtain, he had tried a plan of rotating two films, the one over the other, and thus obtained the neutral tint, so delicate in action that its colour varied by the slightest depolarizing influence of the object examined; and, in addition, gave a display of colours more varied and gorgeous than could be obtained by any of the usual colours employed. At the same time, the rotation of one or other of the films gave a succession of nearly all the prismatic colours, so that, if dissatisfied with the neutral tint, a multiplicity of others could be produced. He had brought with him several selenite stages which could be tested by the gentlemen present.

Mr. Wonfor, in moving a vote of thanks, said they were not only deeply indebted to Mr. Ackland for coming among them, but also for the valuable information he had afforded them.

Mr. Sewell seconded the resolution, which was carried unanimously.

A very large number of polarizing objects of a varied character were then exhibited, with and without the selenite stages, by Messrs. Sewell, Turner, Ackland, C. Smith, Glaisyer, and Wonfor; and general satisfaction was expressed at the results and the increased beauty with the neutral background.

Mr. Hennah also exhibited Dr. Pigott's Aplanatic Searcher, kindly lent by Mr. Curties, and Dr. Maddox's photo-casts of diatoms, ranging from 900 to 3000 diameters, kindly lent by Gen. Worcester.

Mr. Wonfor exhibited a Searcher suggested by Mr. McIntire, consisting of an objective arranged in the draw-tube, with its observing end turned to the eye-piece, and a simple mode of mounting an objective under the stage to be used as a condenser, at any angle, as suggested by Dr. Matthews, and the new slide with movable brass cover for dry objects.

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Beiträge zur Theorie der natürlichen Zuchtwahl von Alfred Russell Wallace. Translated by Adf. Bernh Meyer. Erlangen. Besold.

Dorvillia agariciformis Spiculum.
× 20 lin.

Fig. 1.

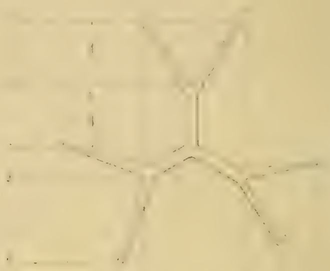


Fig. 2.

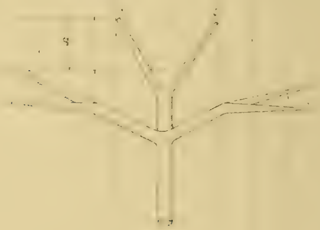


Fig. 4

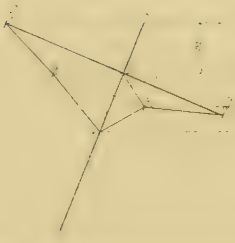
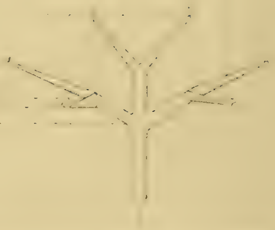
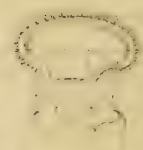


Fig. 3.

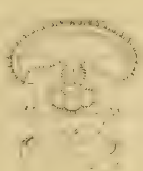


— *Œcistes intermedius* × *130 lin.* —

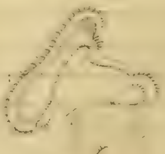
Dorsal
Fig. 5



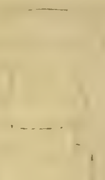
Ventral
Fig. 6.



Lateral?
Fig. 7.



Form of Disc, resulting from fig. 7.
Fig. 8.



C. Cubitt del.

W. West & Co. imp.

Linear Projection.

THE

MONTHLY MICROSCOPICAL JOURNAL.

MAY 1, 1871.

I.—*On the Mode of Working-out the Morphology of the Skull.*

By W. Kitchen Parker, F.R.S., President R.M.S.

(*Read before the* Royal Microscopical Society, *April* 5, 1871.)

A very large amount of information with regard to the structure of the vertebrate skull may be obtained by the use of the ordinary methods, maceration, dissection, &c., with the aid of the pocket-lens. A huge mass of anatomical literature exists in which the development of the skull has been thus traced, say half-way downward. The value of all this is greatly depreciated by the fact that the *first half* of the development processes have not been traced.

Thus we are reading a language only the last twelve of the letters of which have been seen by us. Even as to mere ossific centres, a very secondary matter in the structure of the skull, only about two-thirds of those which may be found in the bird have, until very lately, been described. In my paper "On the Fowl's Skull," published in the 'Philosophical Transactions' last year, the skull is traced back to a stage answering to Professor Huxley's fig. 57, F and F′, in his 'Elements of Comparative Anatomy' (see p. 138). Later researches on the growth of the skull of the common frog have shown me that my earliest stage of the fowl's skull had already undergone a large amount of metamorphic change. I can now understand what those changes were, but to make them plain a series of less mature embryos must be worked out.

For many years my embryonic subjects had been prepared by being hardened in strong methylated spirit; but this never was sufficient in the earlier conditions, and I suffered much loss of time and specimens. Spirit specimens, when examined in section, require to be treated with caustic potass or soda, and then put into glycerine. In this way fine *razor slices* can be examined with the quarter-inch object-glass, and even with the highest eye-piece. I generally, however, use the middle eye-piece. Transverse sections show most; and slices passing from before backwards, and thooughly illustrated and tinted, show very much indeed. I refer again to my paper "On the Fowl's Skull," in which the original figures have been copied in a reduced form and tinted by Mr. Geo. West, for illustration of this remark. But objects hardened by spirit cost much trouble when they have to be used as solid specimens, and examined by reflected light; the tissues are not half hard enough, and they have lost all colour; so that it is no easy work to make a dissection from the outside agree, in the mind, with the various sections that have been made of other specimens at the same stage. Some tadpoles hardened in dilute chromic acid were given me by Professor Michael Foster two years ago; these opened my eyes to the immense advantage of this hardening material. The objects thus procured are very brittle, but with care the most exquisite solid preparations can be made by tearing away the skin, and teasing out the dry, stick-like muscular fibres from amongst the gelatinous tissue which abounds in the tadpole between the skin and flesh. Then the *colour* is so rich; the deep amber tint makes such objects come out in reflected light in a way that leaves the mind quite at ease as to what the eye beholds. Such solid dissections can be made of the skulls of embryos altogether no larger than a mustard-seed, and *solid sections* made both vertically and transversely come out with exquisite distinctness, corroborating quite what had been determined from the outside. An embryo thus hardened can be held between the finger and the thumb, and then the whole creature can be cloven through, "from the nave to the chops," with a sharp razor. Such *vertical* sections are easily made by "an old hand," and so are the transverse slices, to make which I begin at the nose and cut away rapidly as large a number as possible from the same specimen; these are soon *valued* under the inch lens, and the good ones preserved in glycerine between glass, the thick glass receiving a pen-and-ink mark at the time, which saves time whilst investigation is going on. Some of these sections are thin enough for transmitted light; others of almost equal value show well by reflected light.

I do not examine the sections and dissections which take the reflected light in glycerine: I preserve them in it, but always wash it away and put them into water, which allows the reflected rays to pass through much more easily, and gives therefore a much clearer view of the object. I nearly forgot to mention that one most delightful effect of the chromic acid is to give not only the tissues generally an amber colour, but also the abominable, impenetrable "pigmentum nigrum." Often, in olden times, with spirit preparations, have I been almost exhausted with attempts to decipher parts imbedded in this hateful pigment.

A figure in my "Shoulder-girdle" paper, which shows the two halves of the embryo frog's sternum,* cost a week's labour, and but for chromic acid I should have given up Frog-morphology in disgust:—how often have I wished that these little savages—

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* See plate 5, fig. 4, of that work.

cannibals they really are at times—would not lay on their paint to such a degree. That the tissues which go to form the skeleton may be well understood, it is very important that a morphologist should be a good histologist: he not only has to diagnose between tissue and tissue, he has also to depict other structures than the connective-tissue series in their general anatomical relations. Brain, nerves, muscle, glands, skin, mucous membrane—all and every of their various parts have to be seen, pictured, and described in relation. The skull, above all other parts, is the great warehouse and factory where all sorts of materials and all kinds of machinery have to be placed. The skull in its adult form is the most perfect morphological enigma that can be imagined. Luckily the eyes can be gouged out, yet they are fastened to the skull by a pedicel in the sharks and rays; but the ear-sac is always in the way, now loose, now combined, then loose again: it does what it likes amongst the real cranial structures, and never is at one stay amongst the various tribes of the vertebrata.

But the nose is the real difficulty and trouble. The ear possesses its own cartilaginous sac, and although it now marries itself to this and then to the other, yet its ways and doings can be found out with patience; the nasal sacs have nothing to begin with but membrane, and they borrow this and use that; creeping along one part and burrowing into another, so that their very earliest infancy must be known if any account of them is to be given. Thus the skull itself is largely made up of parts having a diverse origin. Essentially, in its first membranous condition, it is a "bleb" blown on to the end of the spinal canal to hold the little sacs that grow out of the front end of the cerebro-spinal axis.

The vesicles of the brain are superadded to the spinal cord, and they have to be lodged in a cavity that grows with their growth and strengthens with their strength. But the upper part of the axis of the vertebrate animal, after subdividing to form the vertebræ, has only an unused piece left, reaching as far as to the pituitary body: about the posterior third of the skull is thus related to the vertebræ, but as an unsegmented piece. From the vertebræ rays grow downwards tending to enclose all the thoracico-abdominal viscera; but under the head *free bars* are found belonging to quite another category of skeletal organs. These carry the gills behind, and become the face, palate, and forepart of the skull, anteriorly. The foremost pair of these bars are called the "trabeculæ," or rafters; they lie above and in front of the mouth, and coalescing after a time with the vertebral remnant at the back of the head, form much of the skull.

The second pair form the arch of the lower jaw, and, sending a *secondary bar* across to the *trabeculæ*, form what is called the "pterygo-palatine" arcade. The third pair of bars are related to the tongue, forming the hyoid arch, and undergoing much metamorphic change, giving rise on their upper part to the little chain of ear-bones—they are only partly bony—whilst a "bung" cut out of the ear-sac, and running to the "stapes" or stirrup of each ear, is articulated with this chain. A free process of this chain is attached to the *membrana tympani*, or drum-parchment. The four remaining arches of the frog's face and throat carry their exquisite tufted gills at first quite free, they then coalesce with each other, and below also to the third or hyoid arch.

All this is microscopic work; step by step everything has to be made out by the most delicate microscopical manipulation, and but for that instrument the whole matter must have been kept secret to the end of the world. It is impossible to overrate the value of such means that lead to such researches, for now we begin to see the absolute Unity of the Vertebrate Series—to say nothing of the other primary groups of animals. The highest type—the human—passes through every stage of morphological structure seen in the series beneath: it does not stop at these stages; it does not utilize, so to say, the incipient structures that are ready to be so used, but runs rapidly along its own line, choosing as it were and refusing, until at length the perfect man is attained. Yet this perfection of parts, this production of a creature who in his lowest attributes is the "paragon of animals," is not brought about irrelatively to the rest of creation; it is merely *an elected consummation* of all that is highest and best in morphological structure. Does this exclude "Teleology," or the fitness of every part to other parts and to the rest of the world? I think not.

II.—*Linear Projection considered in its Application to the Delineation of Objects under Microscopic Observation.*

By Charles Cubitt, C.E., F.R.M.S.

(*Read before the* Royal Microscopical Society, *April* 5, 1871.)

Plates LXXXII., LXXXIII., and LXXXIV.

I have frequently encountered some difficulty in the endeavour to interpret many illustrations of objects "drawn under the microscope," and this difficulty has arisen from a want of coincidence in the representation of different views of the same subject under the varying conditions imposed by a dorsal, a ventral, a lateral view, a view taken from above or from beneath, and stated to have been drawn to the same scale. It has been impossible, with the particulars thus defectively indicated, to interpret the details with sufficient perspicuity to enable me to arrange them in a consistent whole, and to construct a correct drawing therefrom, the one view having proved to be an incorrect representation of another in its altered position; so that while appreciating the exquisite finish in the execution of the works in question, I have been disappointed to find that the drawings have not accurately represented the forms they were intended to portray; and in selecting particular instances as examples of such inconsistencies, I desire sincerely to express to the authors of the same that my sole object is to assist them as fellow-workers, and not to provoke their animosity.

I beg permission therefore first to refer to two figures illustrating Mr. Saville Kent's instructive paper "On a New Anchoring Sponge," *Dorvillia agariciformis*, read at a meeting of this Society on Nov. 19th, 1870, of which Figs. 1 and 2, Plate LXXXII., are reproductions; Fig. 1 being a *plan*, that is to say "viewed from above," and Fig. 2 an *elevation* of a large bi-ternately terminating spiculum × 20 linear; and accepting the *plan* as correct, we find, by placing the elevation immediately beneath it, that in this view, two points only, c and c', are identical with the positions of their coincident points on the *plan*, which will be made manifest by following the vertical lines projected from the one view to the other, Figs. 1 and 2.

The extremities of the branches c and a Fig. 2 could, in their normal state, only assume the positions shown on this figure under the condition of the axis of the spiculum being considerably inclined; and accepting the distance indicated by x' Fig. 2 as a datum or starting point, it represents the horizontal distance between these points indicated by x on Fig. 1, and by *setting off* this distance x, between parallel lines as at Fig. 4, we get the angle at which a plane bounding the said points must necessarily assume, *c*, *a*, and *a'*, as well as the angles of the arms and crutches, *d*, *e*, and *f*, and also the inclination of the axis imposed by such a position, when by projecting horizontal lines from these points to intersect the coincident vertical lines continued from Fig. 1, we see in Fig. 2 where the extremities and bifurcations disagree, and on Fig. 3, as projected, where they are identical with the several points on the plan above, which last is submitted as a correct representation of the spiculum in the position assumed.

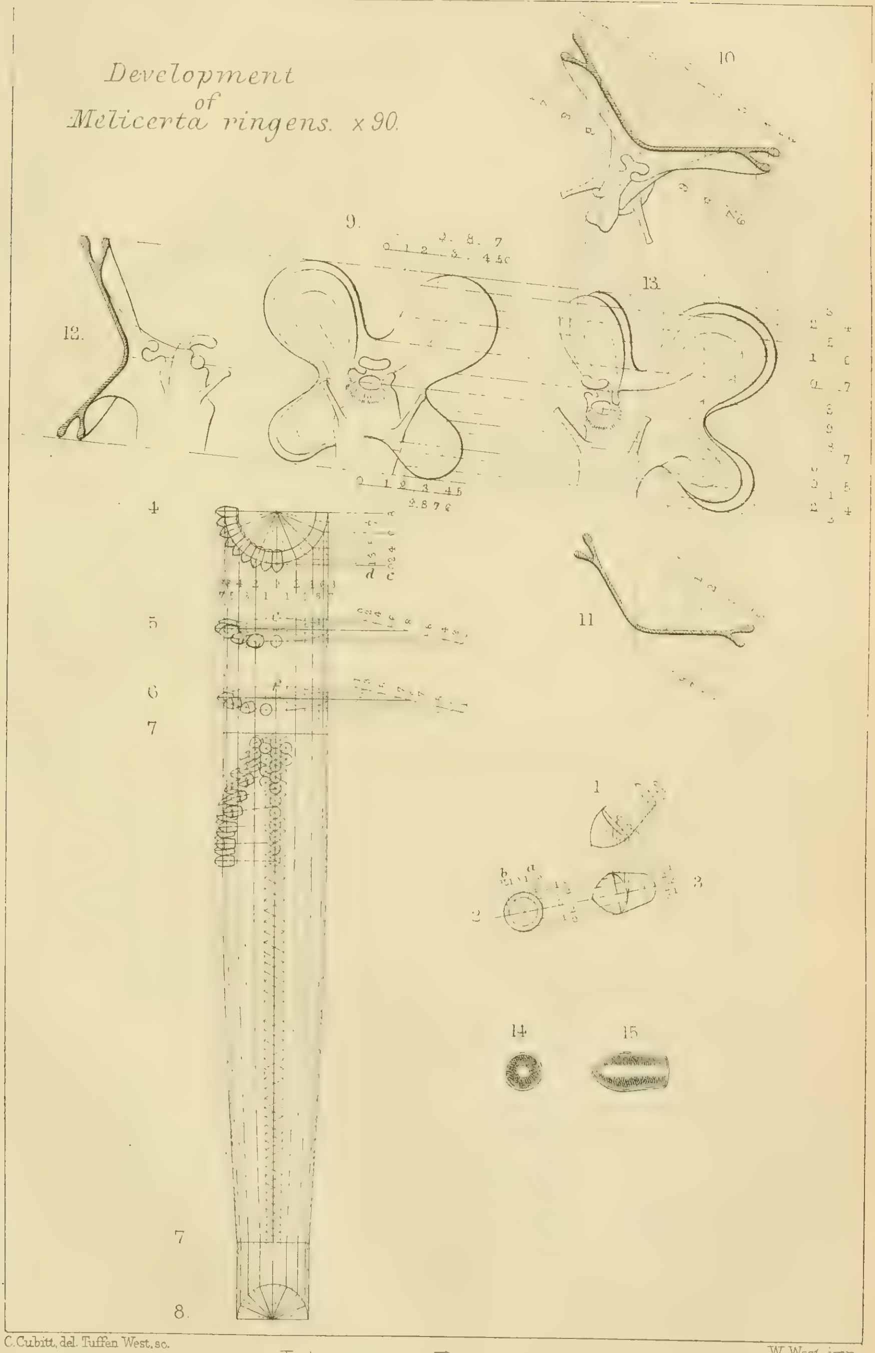
The displacement of the several arms and points in Fig. 2 is just such as would be produced by the employment of a compressor for the purpose of examination, and under such circumstances I presume it has been traced by the aid of a camera lucida; and admitting this to be a correct drawing of the form which was seen "under the microscope," I must beg to submit that it is not a correct representation of that which exists in the tissues of the animal.

In the next place, I have to refer to a rotifer, *Œcistes intermedius*, discovered by Mr. Henry Davis, and described in a paper communicated by him to this Society in the year 1866, of which Plate LXXXII., Figs. 5, 6, and 7, are reproductions of the drawings illustrating the subject; and a comparison of the several views will show that some licence has been taken in creating the figure representing the lateral view of the disk, Fig. 7, for it will be seen that this is just twice as deep as it is shown to be in the other two views, Figs. 5 and 6, which are consistent the one with the other, and that to accommodate such an inconsistency it has been found necessary to *scamp* the length of the tube at the top. Such a lateral view as that of Fig. 7 must necessarily impose in the others an outline somewhat of the form shown by Fig. 8. This discrepancy will fully account for the fact of a drawing of the animal by Mr. Slack having, when placed beside it at the reading of the paper, "differed widely" from the drawing furnished by the author. And I must beg, with all due respect, to dispute the assertion that "both portraits are correct."

On the other hand, we have examples in which the authors manifest a precognition of the principles I desire to inculcate, in the exquisite drawings of Mr. Gosse, illustrating his 'History of the British Sea Anemones and Corals;' and in those of another order illustrating Mr. Hincks's 'History of the British Hydroid Zoophytes,' and even in this last the particular delineation of *Cladonema radiatum*, plate XI. of that work, from the pencil of Mr. E. W. Holdsworth, elevates it in my humble estimation far above any one of the other numerous illustrations of that valuable and highly interesting work.

I do not presume myself to emulate the refinements of the latter examples, but with the view to avoid the inconsistencies

Development of Melicerta ringens. x 90.



C. Cubitt, del. Tuffen West, sc.

Linear Projection.

W. West, imp.

of the former, I have employed a system of Linear Projection, on which I now beg to offer the following explanatory suggestions, not however to those my seniors in the service, but more particularly to younger workers as hints for their guidance, who, while possessing instruments furnished with good powers, and such accessory apparatus as the camera lucida or other reflecting media, invaluable in the case of mounted objects, will nevertheless meet with instances in which such useful accessories fail to afford the assistance required for the correct delineation of certain objects; and I trust that these suggestions as to the process I employ may, under such circumstances, be found acceptable.

In the special instances of tube-dwelling rotifers the difficulty with a camera is insurmountable, inasmuch as the restraint necessarily imposed upon animals under observation at once obstructs that freedom of action which is necessary for their assumption of the expanded state, in which, with their constantly varying attitudes, the employment of any reflector would be wholly inefficient; and with the indifferent aptitude which I possess, regarded from an artist's point of view, I have had recourse to this system as being the most expedient to employ. I first make hand-sketches of the object under observation, and, with the micrometer inserted, *measure up the work*, and then *set it out to scale*, so to speak, and in so speaking I must beg permission to use these *shop terms* somewhat freely in the following remarks as the most expressive that suggest themselves to a *mechanic* impressed with an intuitive predilection for the *wheel animalcules* in treating a subject which is not *scientific*. Should, however, these and other technical expressions, such as may hereafter be applied, meet the objection of scientific *savants* as being *mal à propos* and indefinite, I can only excuse them on the ground of their being most convenient terms, as here employed, to distinguish the different views of an object, and I only trust that with the working members their adoption may be accepted, the process understood, and that their employment will not interpose in elevating the results to a position somewhat higher than that of the mere *mechanical*.

But, before proceeding to describe the process, I must beg permission to specify some of the necessary *tools* and appliances which —in addition to a *good* set of drawing instruments—will be found necessary for the due performance of the work; and these should consist of a carefully-made drawing-board, a T square provided with an adjusting stock, for the purpose of producing a repetition of parallel lines at any other than a right angle, a couple of vulcanite set-squares, one at an angle of 45° and another at 60°, which also affords its complement of 30°, to which may be added a few parabolic curves; but as to these last it will generally be found more expedient to *set out* on cardboard, and to cut therefrom, such particular curves as may severally be induced by the projection of any particular lines in the subject in hand where a repetition of that curve becomes necessary.

I have selected the delineation of *Melicerta ringens* as an appropriate subject for the illustration of this system for several reasons: the *First*, from the fact of its being such a general favourite in the hands of all observers, it may invest the matter under consideration with an interest that will relieve it of what otherwise would have assumed a prosaic character. *Secondly*, that, notwithstanding the complete and elaborate descriptions which have been published on the history of this rotifer, the illustrations accompanying the same are all, so far as I am able to compare, wanting in that perspicuity necessary to convey an accurate impression of the appearance of the animal in its natural condition. *Thirdly*, it will afford a variety of points in the projection of its several details that require a very careful application of this system from the diverse positions assumed by the expanded animal and from the varying angles to which it is prone to subject the different appendages of its head.

The tube, moreover, is a study in itself; and as the several points in reference to the composition, fabrication, form, and disposition of the pellets during the process of construction require to be understood, I beg briefly to recount the most prominent of these points before proceeding to *set out* the details.

The preliminary act in the formation of the tube of the young animal is the secretion of a hyaline gelatinous cylinder round about the body, to which the first crudely-formed pellets are attached at some distance above the foot; these are subsequently forced by sudden contractions of the young animal down upon the support to which the tube becomes permanently fixed, and in some instances these contractions, by reason of the pressure they create, cause a departure from the circular form of the pellets in several of the first laid *courses*, the outlines of which are rendered hexagonal. I find them, however, more frequently to be indefinitely distorted.

The operation of building is carried on thenceforward to the completion of the tube, but on many occasions it may be noticed that the animal having attained its full development persists in lengthening the tube to an extraordinary extent beyond the limits actually required by the animal in its adult condition. Instances of this I have frequently had under observation, in which the tube has reached a height of fully twice that absolutely required by the full-grown animal; and on such occasions *Melicerta* manifests that same capacity for inordinately extending the footstalk which obtains with the floscules, remarkably so in *F. campanulata*, instances of which have attained the extraordinary height of $\frac{1}{20}$th inch from the foot-attachment to the incurved summit of the dorsal lobe, the tube at the same time reaching fully up to the neck. Similar instances occur with *F. cornuta;* and that this inordinate extension with *Melicerta* is also confined to the footstalk we receive confirmation in the fact, that during acts of evacuation when the cloacal orifice is brought to the verge of the lengthened tube, the body of the animal retains its normal proportions; this abnormal attenuation being consequently confined to the footstalk.

Much discrimination is exercised by these clever artificers in the several processes they adopt in the acts of *brick-making* and *brick-laying;* the heterogeneous mass that is first brought about the disk by the force of the ciliary currents is seen to pass beneath a pair of small ciliated processes, which exercise a rapid discrimination in the selection of those specially adapted for alimentary purposes and others for the formation of these pelletoid bricks, which particles, when so selected, are seen to emerge from beneath the two ciliated processes which conduct this operation, and to course along the *chases* beneath the lateral margins of the dorsal lobe downwards into the pellet *chuck* or *mould*, where this granular matter becomes blended with a cement, secreted during the process of manufacture, and are moulded by the action of the contained cilia, by which they are also *shaped* and *finished off* in the peculiar form, resembling somewhat that of the pellets of the Minié rifle, with this difference, that the sides forming their bedding surfaces are not parallel, but radiate with their axes from their shoulders to the centre of the tube.

The pellets are not formed of one homogeneous consistency, but preserve a central *core* through the granulate matter of which they are composed, this core is perfectly transparent on an end view as represented by Plate LXXXIII., Fig. 14, and a side view which, by careful adjustment of focus, presents a horizontal section such as shown by Fig. 15. The *bedding surface* maintains a tolerably constant form and proportion, while the more abruptly tapering outer ends are seen at times to deviate considerably from their usual shape, and frequently to assume an extraordinary degree of attenuation.

These pelletoid *bricks* are laid in a peculiar manner, they are *bonded* in their horizontal *beds*, and *break joint* the one with its neighbour, so that while there are thirty-two pellets in the whole circumference, there are sixteen only in each horizontal *course*, presenting in elevation perfectly straight and unbroken lines, inducing by such a disposition a series of helices around the periphery of the tube when the tube is viewed vertically, but any departure from that position modifies the character of the curve more or less, according to the degree of inclination, as in the instance before us.

This particular disposition of the pellets in the construction of the tube ensures great strength, and is eminently calculated to resist the strains to which it is so frequently subjected from the concussions of her roving compeers, which horizontal *courses* would be less competent to withstand, and the first formed portion of the tube, being more densely blended with the gelatinous cement, assists it, by reason of the resilience so afforded, to yield somewhat to such shocks, which at this particular point produce the greatest effect in tending to rupture the tube.

It will be readily comprehended that the foregoing particulars impose very delicate manipulation in the several processes involved in *setting out* the tube, more especially as it is intended in the present instance that the top of the tube shall incline somewhat towards the observer, and as this condition governs the outlines of every individual pellet of each and every *course* throughout its whole length both of the parallel and tapering portions, it demands our special and attentive consideration; and as the preliminary operations require to be clearly defined, I will take as a first example a single pellet more highly magnified, lying on the plane imposed by the inclination of the tube, and inclined at an angle of 45°.

First draw the pellet in outline on *plan* at the required angle, Fig. 1, Plate LXXXIII. Next describe circles representing the diameters of the shoulder and small end, Fig. 2; then divide one-half the circumferences into any convenient number of points, taking six in this instance, of a regular form, which will be given by the set-square of 30° and 60° (but varying in number and disposition in irregular forms according to circumstances), identify these points on Fig. 2 by numbers from the central point 0 to 6 consecutively, and from each point draw parallel lines extending indefinitely in the direction in which it is desired to show the finished view; then project the points 1, 2, 3, vertically on to any horizontal line for each end *a*, *b*; prick them off on the edge of a slip of paper and transfer them on to the *plan*, Fig. 1, when by projecting vertical lines from each of these numbers from Fig. 1, their respective intersections with the coincident lines from Fig. 2 will give certain points that limit the outline of the upper half of Fig. 3, and the lower half being simply the reverse of this, the continuance of these vertical lines, at right angles with the axis, to their intersections with the horizontal lines 4, 5, and 6, will complete the figure of the shoulder of the pellet; the small end and tapering point being set out in a similar manner.

The projection of the complete tube now claims our attention, and this should be proceeded with by drawing *half plans* of the top and bottom, Figs. 4 and 8, at convenient distances from the position to be taken by the *elevation*, Fig. 7, first by describing semicircles cutting the shoulders of the pellets, and to set out accurately on each of these their radiating axes, when for the sake of distinction it will be advisable to indicate the positions of the horizontal row by full lines and those of the other by dotted lines; number them consecutively from the central point 0, and project these data severally on to planes bounding the top and bottom of the *elevation* required, Fig. 7, when by connecting the points of the top with the corresponding points at the foot, we arrive at the central positions of each *vertical* row along the whole length, Fig. 7.

As the transverse disposition involves a duplicate system, it will be necessary to *set out* a diagram of each layer separately, Figs. 5 and 6, by constructing the *curve* these transverse courses assume from the inclination to be given to the tube, which will be determined by projecting them horizontally from Fig. 4, *c* and *d*, and transferring the same, each on to a plane representing the top of the tube at the angle induced by the stipulated inclination, as shown on the right of Figs. 5 and 6, when the intersections of the horizontal lines from each of these points, 0 to 8 for the upper, and 1 to 7 for the lower row, with their coincident vertical lines from the *plan*, Fig. 4, will give their central positions taken at their shoulders, forming the limits of the said curve assumed by the horizontal layers; then by striking radii from the centres *e* and *f* in each individual instance, we obtain the axial angles of the pellets, upon which the several outlines for one-half of the *elevation* have now to be individually projected, as shown on the left of Figs. 5 and 6.

On the careful execution of the foregoing preliminary operations, as well as on the accurate projection of every individual pellet on each axis, must depend the integrity of the finished work. It will be seen that while the outlines of those of the central row are circular, those of every row beyond depart by minute but definite gradations from the outline of a circle to that of the forms inscribed on the *plan*, Fig. 4, and although those next adjacent to the central row may pardonably be represented by circles also, it leaves no less than seven different and distinct forms, each at a different angle, to be individually projected for one-half the *elevation*, in the same manner, though not necessarily with the same minutiæ, as described for the enlarged view, Figs. 1, 2, 3.

The pellets of the three central rows having been described by circles, those of the next adjacent have to be alternately depicted by repeating the several radiating axes obtained on Figs. 5 and 6, down the whole length of the tube; their points may be determined by means of vertical lines from the plan, and their shoulders being at right angles with their radiating axes should be set off at the several intersections of these axes, with the vertical lines first projected from the plan, when their diameters may be *squared off* from the central line of pellets, by which means we obtain the points which limit the several outlines all down the tube; the centres in the one row, and the peripheries in the other, answering as data for this purpose without involving any measurement whatever, when it only requires the operator to *fill in* at his discretion their respective varying outlines. And here the shifting stock of the T square comes to our assistance.

The varying proportions of the pellets in the tapering portion of the tube would at a glance seem to impose the necessity for a tedious repetition of all the foregoing operations; but *nil desperandum*, the central positions of any two pellets in their peculiar disposition are precisely equidistant from their neighbours right and left, as they also are from each other vertically, or would be so on a plane surface, the difference due to the circular outline of the tube being in this instance inappreciable, so that the centres of the vertical and horizontal courses of the pellets form together a double series of equilateral triangles all down the tube, whether parallel or tapering, and by the employment of the *set-square* of 30° we obtain their correct central positions and resulting diameters all down the whole length of the tube, thus determining the data from which all the others have to be projected and *set off* more accurately than could be attained by any method of *scaling*.

The projection of the trochal disk embracing the four frontal lobes with their marginal cilia and supporting ligaments, the secondary range of decumbent cilia terminating in the pellet cup beneath the *dorsal* lobe, and the two conspicuous antennæ though less complicated, necessitate an equally careful application of this system, especially where, as in this particular instance, it is intended to show the animal at the same inclination as its investing tube, and inclined also in its horizontal position, so as to relieve it of the too formal and diagramatic character imposed by parallelism, and having selected such an attitude, the several diagrams which have been constructed from actual measurements, are arranged in the several positions shown in Plate LXXXIII., which are necessary to produce the finished form; Fig. 9 being a dorsal view, Fig. 10 a plan of the superior lobes, Fig. 11 a plan of the inferior lobes, and Fig. 12 a lateral view; and it only requires the several intersections of the coincident points from the different views to be followed, to trace the outline and prominent features in Fig. 13, which is a rough outline of the finished drawing, Plate LXXXIV.

Errata in last Paper.

Owing to certain exigencies which obstructed the proper correction of the proofs of my paper "On the Winter Habits of the Rotatoria," I desire here to explain that page 168 line 10 from the bottom should read Clustered Free; and line 4 on page 169; by means of the *currents* it creates.

— *Melicerta ringens* × 90 —

J. Cubitt del.

W. West & Co. sc. et imp.

III.—*Optical Appearances of Cut Lines in Glass.*

By Henry J. Slack, F.G.S., Sec. R M.S.

(*Read before the* Royal Microscopical Society, *April* 5, 1871.)

The use of high powers in delicate investigations renders it necessary that the microscopist should study the character of appearances which arise from optical laws, and which can only be rightly interpreted by referring them to forms and structures to which they bear no real or exact resemblance. A short time since the writer called the attention of the Society to the deceptive nature of the appearances presented by fine cracks in silica films; and further observations show that if the finest or narrowest of such marks are selected for examination, the chances of obtaining perfect illusions are increased by the amount of magnification and the perfection of the objectives employed. Delicate interference bands, pseudo-beading, &c., look more real with well-corrected object-glasses than with bad; and careful illumination will often add to the *structural* aspect of mere optical effects.

The edges of silica cracks, obtained as mentioned in a former paper, differ from the edges of minute furrows cut in glass, by being smooth instead of jagged. The latter as well as the former are well worth study. Preparatory to examining such furrows as are cut with diamonds in glass for micrometers or diffraction gratings, it is well to notice the edges of thin glass cut for slide-covers. If half-a-dozen or more thin glass squares are held close together,* and viewed, edges upward, as transparent objects, a variety of curious optical effects will be seen, arising from interfering reflexions and refractions. The examination should begin with an inch or ⅔rds, after which ½ inch, and ¼ or ⅕th will be advantageously employed. It is easy to focus parts of the glasses' edges, so as to show their true form; but portions a little in or out of focus will show beads, appearances like columns of Egyptian architecture, &c. Most of these optical appearances are sufficiently hazy or confused to give warning of their true nature; but generally some will be found so sharp and clear that, if viewed separately, they might easily mislead a practised observer. In making these experiments, it is best to have handy a box containing at least several dozens of the thin glasses, as some sets will prove much more interesting than others. They should be viewed with their edges parallel to the plane of the objective, and also at various angles. The corners of the squares should also be looked at.

Lines cut in glass for micrometers or diffraction-gratings are usually filled up with finely-divided black-lead, and the same material has been employed in the writings and patterns made with the Peter's machine. This substance of course modifies the appearances. To see them in a simpler form recourse was had to Mr. Ackland (Horne and Thornthwaite), who ruled several sets of fine lines, each on glass slides, at varying distances, 1—2000″, 1—3000″, and 1—4000″, and mounted them with Canada balsam, so that they could be safely used with immersion lenses. One set was not covered or mounted in any way.

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* The stage vice made for me by the late Mr. Thomas Ross is very handy for this and many other purposes, as the blades approach each other in a parallel direction, and can be fixed at any distance with great nicety.

Those who have examined very minute writing done by the late Mr. Farrants with the Peter's machine will be aware that even when a very fine diamond-point is used, the incision partakes more of the character of a scratch than a clean cut. It seems impossible to cut glass with a smooth, clear edge, such as certain metals readily give with a sharp tool. A line cut in glass is thus a furrow, more or less rough at the bottom and sides, and when viewed correctly under the microscope, has the appearance of a narrow depression less transparent than the adjacent spaces. It is difficult to get a really correct view. Even under favourable circumstances of illumination and correction, the edges of a cut are apt to appear as two raised lines.

Many instructive optical appearances, which might bewilder the observer if the true character of the object were not known, may be easily produced, as the following notes will show. The observations were made with Powell and Lealand's immersion $\frac{1}{8}$th and Ross's $\frac{4}{10}$ths, condenser aperture 109°. Using central stop A, and varying inclinations of mirror. Paraffine lamp.

(1*a*). Cuts as rounded bands; interspaces flattish furrows. The bands illuminated on right side, shaded on left. Tint of lightest part of furrows bluish.

(2*a*). Flattish bands and rounded furrows, the former slightly shaded on left; tint of shading bluish.

(3*a*). Oblique rounded furrows with narrow blue ridges; broadish bands with narrower elevated bands up their centres, light on right side, shaded deeply down the furrowed side on left.

Same condenser 109°, two radial slots forming obtuse angle. Angle of mirror varying.

(1*b*). Broad, flat spaces, narrow, shaded, and elevated ridges.
(2*b*). Ridges four times as wide as No. 1, with rounded tops.
(3*b*). Narrowish grooves, *something* like actual object.
(4*b*). False ridges, puzzling to count, and hollow.

Same condenser 109°, two rectangular radial slots. Angle of mirror varied.

(1*c*). Half-round hollows, with rod-like ridges in the middle; rounded interspace elevations somewhat lower than ridges and between them.
(2*c*). Narrower ridges; nearly flat spaces.
(3*c*). Appearance of additional ridges, strongly shaded on left.
(4*c*). Narrow ridges, shaded on right; flattish spaces, and low ridges, with narrower shelving shade-spaces down to ridges, &c., &c.

Same condenser 109°; one radial slot, which was rotated to various angles. Angle of mirror varied.

(1*d*). Each cut made into a flattish space, with two narrow, raised edges, shaded on left.
(2*d*). Cuts made into flattish, ribbon-like elevations, with raised edges.
(3*d*). Interspaces raised, with rounded edges; cuts made to look flattish, and at lower level.
(4*d*). Appearance of additional and imperfect ridges.
(5*d*). Series of imbricated and shaded bands.

In the lines cut by Mr. Ackland no attempt was made to produce the narrowest possible furrows. The width of furrows found practically convenient for micrometers was only slightly deviated from, as some cuts were a little deeper than others, and thus caused the wedge-shaped diamond-point to open the top of the furrows a little wider. The interspaces of the narrowest were much wider than the cuts. It is obvious that a cut wide enough to be distinctly seen, under given magnification, will present to view *two* linear edges, and thus be reckoned as *two* lines if its true character is not considered. Cuts very close together may, if the cohesion of the glass and the perfection of the cutting tool permit, be wider than their interspaces.

It will be seen that in the preceding statements only one instance is mentioned of appearances agreeing tolerably well with the real facts. It must not be inferred from this that it is not easy to exhibit moderately-fine cuts correctly, or very nearly so. The object of this paper was to select a number of appearances all looking as if they might correspond with the facts, and all differing more or less from them.

Those who study the most vexatious diatoms or Nobert's test-lines must, it appears to the writer, not only take into account what they do see, but what they ought to see, provided the object has a certain definite structure, and certain powers of producing optical images under given conditions.

IV.—*Object-glasses and their Definition.*

By F. H. Wenham, Vice-President R.M.S.

Under this title I have no wish to trespass on the patience of the readers of this Journal. I must, however, remind microscopists that until quite recently they have been kept in practical ignorance of the tools they have been working with. Much valuable information has been hitherto exclusively in the hands of the principal makers of object-glasses, who years ago, under an implied confidence that I had no immediate intention of divulging any point that they held as a secret, did not hesitate at a mutual exchange of information. This time has passed away, and the number of makers that have since appeared render it no longer possible or desirable that such knowledge should be confined to the few, but, in order to extend the march of improvement, that the question of object-glasses should now be thoroughly discussed in all its optical conditions and practical bearings.

From its recent controversial character, some of the information has been adduced in rather a desultory manner; at present this can scarcely be avoided, as a few of the questions mooted still remain unanswered. The first relates to the Podura. The return of temperate weather has brought out the insects, and I have now no difficulty in finding various specimens. I have torrefied some of the scales for the purpose of gaining some further evidence of their structure. The method is as follows: the insect is placed on a piece of velvet, and the thin-glass cover laid on its back. In its struggles to escape, a quantity of scales will be detached and adhere to the glass. This is then laid (scales uppermost) on the end of a piece of thin bright steel (an old worn table-knife will answer well), and held over a candle till the steel turns to a very dark blue or purple. The scales will at this heat be completely scorched. Of course every time the operation is repeated the knife must be repolished, in order again to see the right temperature by colour. After this treatment the scales in a great measure lose their transparency, so that we are far less liable to form erroneous opinions of structure arising from false refractive figures.

Viewed by the dark field illumination, as obtained by the parabolic condenser, they no longer appear of the characteristic blue colour, but are of a reddish-brown tint. The markings are most decidedly plainer as seen thus opaquely. Most of the scales have become more or less distorted and curled, like parings of horn or dried-up leaves. Both at the longitudinal bendings and inclined sides most satisfactory perspective and profile views may be obtained of the ribbings or markings, all of which, on both sides of the scale, run longitudinally without any oblique crossing, and still retaining their clubbed form; and from the greater distinctness that is now given to the *membrane* by the destruction of its transparency, the ribs or markings can be plainly distinguished as surface elevations. From the brittleness of the burnt scales (which have been heated nearly to ignition), they are easily fractured. When this happens longitudinally, it takes place close to the markings, and follows their shape, showing that they form a ridge or material barrier for the line of direction. When the fracture is transverse, the ridges again offer an impediment as the parting is not a straight one, part of the markings projecting over and leaving a corresponding notch in the separated portion. In fact Podura scales treated in this way leave so little doubt of their structure, that I think that no one that will take the trouble to try this simple experiment will admit Dr. Pigott's interpretation. The scales can be seen in every possible position; some of them even curled up point and quill almost together, thus affording an end view.

That remarkable Podura scale, the *Seira Buskii*, alone appears to afford the key to their structure. The ribbings are somewhat prominent, and the ! ! ! markings so much elongated that only three or four of them occupy the length of the scale, which is also very convex, and the intercostal spaces much wider than in other specimens. By no means of illumination, or any stretch of imagination, can these markings appear to be caused by the oblique crossing of ribs on opposite sides. There are some coarse transverse membrane corrugations similar to those which in the finer specimens give rise to illusory beadings.

Let us not forget how deeply we are indebted to Dr. Pigott for promoting these researches, who, by the power of negative inferences, has elicited so much practical information, and who first discovered a waved structure in certain Podura scales, and repeatedly demonstrated that this might be resolved into ghost beads appearing as substantial as "peas in a pod."

"Illumination is a vexed question for the photographer, who has never yet succeeded in displaying the spherules of the *Amphipleura pellucida*, which so many English observers have seen microscopically. Unless, therefore, the defining power of photography at least equals the human eye armed with the microscope, no reliable argument can be drawn from its failure or approximate revelations." Thus again did the dictum go forth, and shortly after its echo had crossed the Atlantic, the genius of truth was invoked, and with the sun for a limner, the desired result achieved, and so exquisitely sharp and distinct are the lines on the photograph, thus skilfully obtained by Dr. Woodward, that I am tempted to ask the question, Has anyone ever *seen* them directly through the microscope as plainly as this? *

Again was the immersion lens delineated in the region of fancy and romance, and then, by the application of a few commonplace measurements taken from the real thing, it became known that it had no property for collecting from a balsam-mounted object a greater number of rays, but that the limit is the same as in the dry lens.

"As it seems right in the eyes of some of the old lovers of dry objectives to dispute a number of things that they did not learn in their youth, it may not be out of place to quote a principle at which advanced optical students are quite *au fait* * * * * It would seem that these writers must be unacquainted with the standard works on optics, and so of course could not be expected to know that the angle of total internal reflexion," &c., &c.

These have been noticed by others as specimens of good taste and literary courtesy; I, to whom they were applied, record them but to forgive them, with the desire that their integrity may rest upon the merits of work past or to come.

I have carefully examined the aplanatic searcher made under Dr. Pigott's directions, and * * * * but avoiding all expression of disappointment at the result, I add my voice to those that take the will for the deed, and exclaim, Let us be thankful! †

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* I have again to thank Colonel Dr. Woodward for a series of twenty photographs, which are very remarkable specimens of clever manipulation. Ten of the Podura are illuminated to obtain the beaded appearance, which is most plainly shown in the scale known under the titles of "Black Podura," "Speckled Podura," *Degeeria domestica*, *Seira domestica*, *Degeeria nigromaculata*; but, as I have before remarked, the "beads" are developed at the sacrifice of that sharpness and distinctness of outline associated with the note of exclamation markings. No. VIII. is most distinct, in which the ! ! ! appear with the "beadings," or, rather, corrugations, not on but *between* them.

† "Great things for science have been achieved by means of the microscope, but these will now be outdone by the aid of an apparatus which the inventor calls 'An Aplanatic Searcher,' and which, when applied to the microscope, increases its power, its penetration, and capability of definition to an almost incredible degree. Objects, which under the best of ordinary microscopes appear as black patches, are seen to be full of beads, or lines, or grooves, or possessed of a fashion of some sort, with the aplanatic searcher. Some theories of vital organization are built on discoveries made by the microscope; and if these discoveries now prove to be delusions, the theories will have to be abandoned or re-written. This is especially the case with the 'germ theory" and the theory of spontaneous generation. The minute disk of jelly in which the germ was supposed to lie is now proved by the aplanatic searcher to be a delusion—a false image—due to nothing more than the imperfection of the object-glass. From this it will be understood that a revolution in microscopical science may be looked for. The inventor of this new and searching apparatus is Dr. Royston-Pigott. A full account thereof will shortly appear in the 'Philosophical Transactions.' Meanwhile, some particulars have already been published in the 'Proceedings of the Royal Society.'"—*Chambers's Journal*, Dec. 31, 1870.

I have been trying a number of experiments with various forms and combinations of lenses in different positions between the eye-piece and object-glass, but I may state that I do not profess to have got exactly a similar arrangement to the noted searcher. I cannot arrive at this without incurring the risk of making a spurious imitation.* But, before entering upon the land of promise, it will be as well to note something of what has already been done in this direction.

At the time when the first achromatics were introduced, Dr. Goring attempted to obtain achromatism by various combinations and forms of lenses, intended to act as correctives, placed at intervals between the object-glass and eye-piece. He admits that he failed; nor was success possible, as the arrangement is faulty in its very principle. From an uncorrected lens or object-glass an immediate divergence takes place between the most and least refrangible rays or the violet and red end of the spectrum, the angular separation of the two rays of course increases with distance, and at length any form of concave lens will be quite incapable of re-combining them. It is for this reason that small concave lenses of dense flint have never been successful when placed half-way down a telescope, having a single object-glass of crown. The reason for attempting this plan was to avoid the expense and difficulty of obtaining large disks of flint glass free from imperfection. Even an excess of thickness in the back lenses of a microscope will show an appreciable amount of uncorrected colour, and they are worked as thin as possible in order to avoid this error; for artificial achromatism can only be obtained by the *abrupt* refraction between two adjoining surfaces. We cannot imitate the perfection of nature in the eyes of animals, wherein colour is corrected by a gradual increase of density. Dr. Goring's last achievement was a low-power form of microscope, a description of which was not published till after his decease; this he termed the "Megaloscope." The instrument was arranged for the purpose of obtaining a large and flat field; the eye-piece was the ordinary Huygenian, of low power, and the object-glass proper was composed of two similar achromatics; but the peculiar feature consisted in a third achromatic of shorter focus than the others, set in a racked tube. By separating this, or bringing it quite close to the others, the power of the instrument could be instantly doubled. About the same time Mr. Lister made a series of experiments in this direction, which resulted in the well-known "erecting glass." This consists of two single plano-convex lenses of $1\frac{1}{4}$ and $2\frac{1}{2}$ focus, with an interval of three inches; the shorter focus and flat sides are next the object-glass. By means of the ⅔rds object-glass and draw-tube a power from 5 to 150 may be obtained. From the crossing of the rays and the formation of an intermediate image, colour correction was not impaired throughout, though for the best definition Mr. Lister recommended an increase of distance between the two lenses in proportion as the draw-tube was extended, and a means was provided for effecting this. With a 1½-inch object-glass, as the tube is closed the anterior focus becomes lengthened till its distance is infinite, giving all the grades between a microscope and telescope, and distant objects may be drawn with the camera lucida. A form of dissecting microscope was manufactured by Smith and Beck on a modification of this plan. The draw-tube was moved by a rack and pinion. On viewing some injections with this instrument I was struck by its extreme handiness; for, by keeping one hand on the lengthening and the other on the focussing pinion, all the ranges from a low to a high power could be instantly made with one object-glass, and the extent of field gave a great advantage over the plan of changing the eye-pieces, and the images were sufficiently "*aristokratic*" to make it well worth while to attempt to improve the arrangement, and to ascertain how far one object-glass of large aperture may be made to do the work of several powers, for a cheap form of instrument.

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* Should a demand render it worth while for any leading optician to construct the searcher, for his own credit's sake he would do it properly. But how is this possible, when no illustrative description has been given from which it can be made?

Since the revival of this question I have made a number of trials of the effects of intermediate lenses, either under or over-corrected, in neutralizing any errors in the object-glass. Chromatic correction cannot be obtained this way for the reasons stated, and out of the numerous combinations that I have tried none have any material influence in improving the oblique pencils; a ⅕th with outward coma had outward coma still with all. If the object-glass is carefully corrected by the adjusting collar on a covered object, and any intermediate lens or lenses are then introduced, that correction will no longer serve, but has to be altered, as the searcher (so to term it) *itself induces an error of correction in the object-glass*, by shortening the posterior or conjugate focus and extending the anterior one, and the aperture or angle of rays for which it was specially corrected entering at different incidences to those previous to the introduction of the intermediate lenses, positive aberration * is caused; this requires for every difference in their position another alteration of the adjusting collar. Sometimes the range of this is not sufficient to produce the required correction—the "searcher" may find a position with an object-glass improperly corrected or adjusted, or with no means of adjustment at all, in which the definition may be improved; but in cases where a good object-glass has to be thrown out of adjustment for the sake of accommodating errors from superadded lenses, the investigation does not hold out much promise, and I regret that in no instance have I been able to define any description of error in object-glasses that may lead to their practical improvement, by the aid of such intermediate lenses, as these derange the very corrections that the optician has been striving to make perfect. Enormous magnifying power may be gained by such lenses, but in effect this is far inferior to that obtained directly by high power objectives, and may give rise to illusory appearances. The inquiry is by no means exhausted, and something may yet come of it in the shape of an universal microscope, as an improvement upon that manufactured by Smith and Beck. This was almost exclusively used with a $\frac{2}{3}$rds object-glass, rather differently corrected with an oblique-cut adjusting collar which gave a considerable range (near $\frac{1}{4}$th of an inch, if I recollect right) between the lenses, which were to be separated in degree as the draw-tube was extended.

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* Dr. Pigott defines positive and negative aberration as *over* and *under* correction. Fact is the reverse. This may, perhaps, be attributed to a printer's error.

In a recent trial I have found that with a $\frac{4}{10}$ths or $\frac{1}{5}$th, an erecting glass or intermediate combination consisting of two achromatics of 1 inch or $1\frac{1}{2}$ inch focus with an interval of about $1\frac{1}{2}$ inch between them has given the best result. The shorter focus or one nearest the object-glass should be considerably under-corrected, and the other over-corrected to a counteracting degree. By altering the distances between them, the second image formed by the arrangement can be improved for the aberrations, which vary for every position that they occupy in the body of the microscope.

The foregoing was written previous to the appearance of the last number. In reference to actinic glasses, I may state that when I practised micro-photography some eighteen years ago, I found it necessary to use object-glasses in which the visual and chemical foci were coincident in one plane: this I accomplished by adding another single lens set close behind the others, of a focus suitable for obtaining the result. Colonel Dr. Woodward in using the *dry* lens, replaced one of the combinations by another, giving an identical visual and actinic focus. He states in his recent paper "that the immersion objectives of high power require no special corrections to fit them for photographic use, and are the very ones actually employed." This is a valuable property of the immersion lens that has not before been noticed and is correct in theory, for the adjustable thickness in the front, which the water film permits, is self-compensating, and will at once produce the required under-correction for coincident foci, so that no photographic lines are separate from the visual ones, and Dr. Pigott's authority that errors may arise from this cause cannot be accepted.

V.—*Transmutation of Form in certain Protozoa.*

By Metcalfe Johnson, M.R.C.S.E., Lancaster.

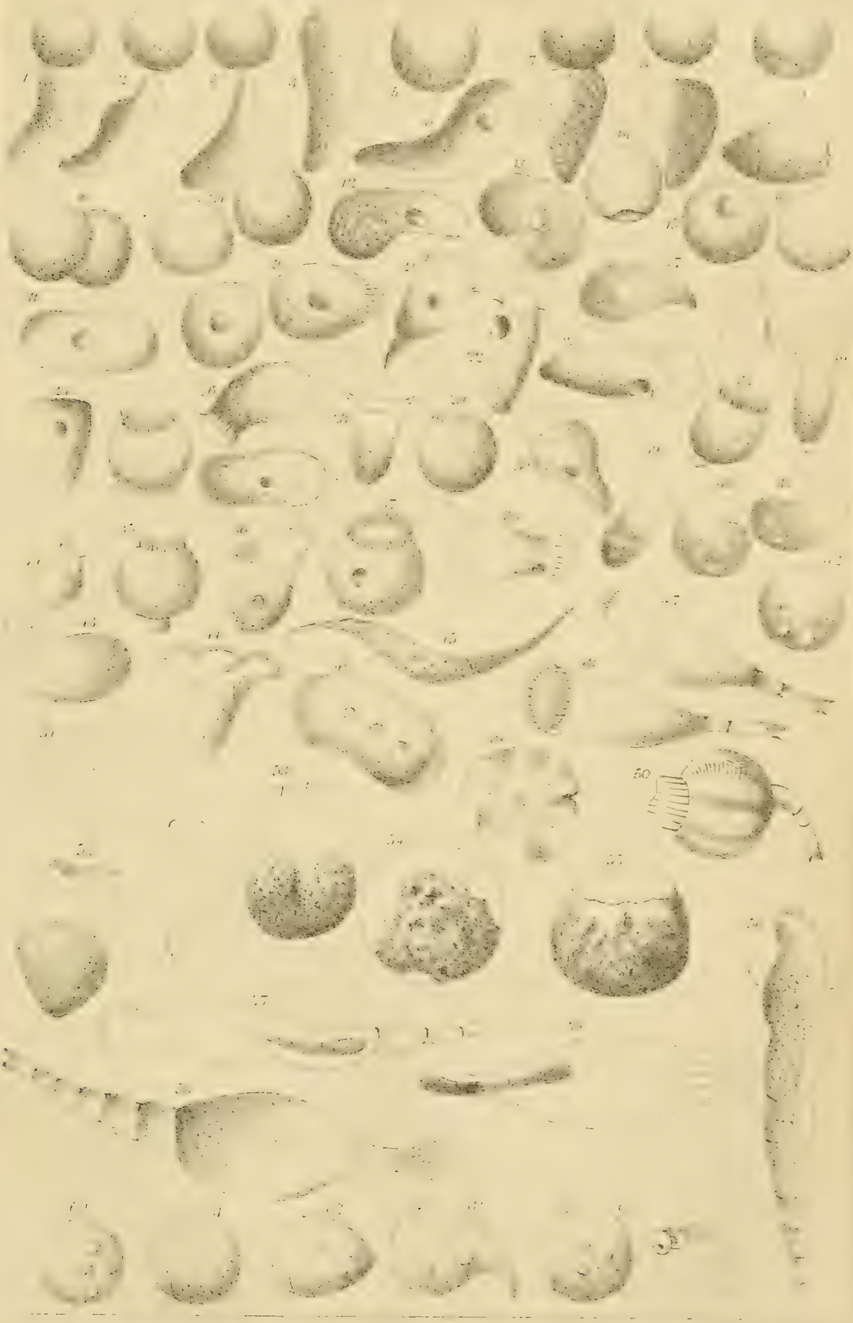
Plate LXXXV.

In the 'M. M. J.' for April, 1870, I have ventured to remark that "Monas and its congeners became at once important as agents in removing dead cells, and in their place supplying us with *green* verdure which is springing up around us on every side." Everyone must have observed that universal greenness which, after the lapse of a few weeks, spreads more or less over every weather-exposed surface, large or small. Sir Humphry Davy, writing forty years ago, says, "A polished surface of a building or a statue is no sooner rough than the seeds of lichens and mosses which are constantly floating in the atmosphere make it a place of repose, grow and increase." If we examine a few of the green growths upon these surfaces differing from one another in their surroundings, or "choses extérieures," such as moisture, light, temperature, &c., we shall find one composed of a green dust, to which the name of *Chlorococcus* has been applied; another, a green scum upon the surface of a liquid, which has received the name of *Euglena;* a third, forming patches of dark green slime upon old walls, and called *Oscillatoria;* a fourth, *Lyngbya;* a fifth, *Vaucheria;* a sixth, *Schidzonema*, and so on. A more detailed examination of these separately-named products, and a study of their life-history, leads to the opinion that they are all (more or less) stages of development of some one common source, which it is the object of the present remarks to identify as the monad, or pin-point source of life, which has been pointed out by Dr. Bastian and others as the earliest form in which we recognize living matter.

In a review of nature we are insensibly led to observe an apparent unity of all its parts into a continuous whole. Thus, in tracing black to white, we find no point of divergence perceptible to the senses. Positive electricity only differs relatively from negative. The animal differs from the vegetable, but the difference is insensible. In order to establish a view of nature such as is here proposed, it will be necessary to inquire what evidence we have before us—first, of a general, and, secondly, of a particular kind.

Professor Graham, in his 'Liquid Diffusion applied to Analysis,' asks, "Can any fact more strikingly illustrate the maxim that in nature there are no abrupt transitions, and that distinctions of class are never absolute?"

In an article called "Higher and Lower Animals," already referred to in this Journal, it is said, "Rather may animal life be likened to a great tree with countless branches spreading widely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from a common trunk, and drawing their origin from a common root, branches bearing all manner of flowers, every fashion of leaves, and all kinds of fruit, and these for every use.

"Professor Haeckel looks upon the causes which have led to the present diversity of living nature as twofold. Living matter, he tells us, is urged by two impulses—a centripetal, which tends to preserve and transmit the specific form, and which he identifies with HEREDITY, and a centrifugal, which results from the tendency of external conditions to modify the organism and affect its adaptation to themselves."* According to M. Famitzen, the gonidia of lichens, if maintained in a condition of humidity on the surface of bits of bark for several months, will give rise in their interior to zoospores, that is, to uniform corpuscles provided with definite movements by vibratile cilia like the zoospores of Algæ.†

Mr. Browning‡ says, "Many of those who are still sceptical as to heterogeny admit that they have watched the conversion of bacteria into fungoid growths, and some have even alleged that they have witnessed the conversion of bacteria into infusoria."

We find in the natural history of *Protococcus pluvialis*,§ "The motions of the various forms of Hæmatococcus are there described, and appear to be very like those of *Euglena viridis;* in fact, so much so that Flotow himself compares them with those of *Astasia pluvialis*."

Griffith and Henfrey say,‖ "The forms included under the family thus characterized are still very imperfectly understood, and it is probable that some of them, separated generically by Ehrenberg, are only transitional conditions of others."

Harvey says,¶ "We might readily take for a Protococcus or other simple Alga what are only the spores of a conferva.

"The granules of which the green matter is composed detach themselves from the mass one after another, and having thus become free they move about in the vacant space of the joint with an extreme rapidity."

Professor Williamson, speaking on *Sphærosira volvox*,** says, "In this stage of its development each protoplasm bears the closest possible resemblance to an Euglena. The motile condition of the *Protococcus pluvialis*, I am convinced that if one of the gemma of Sphærosira and an individual *Euglena viridis* were placed side by side they would be wholly indistinguishable."

In order to commence this investigation I will append a few observations I have made on various forms of *Paramœcium*, and I shall endeavour to show that it constantly transforms to *Vorticella*, and thence passes to *Callidina elegans*, thus tracing one of the phases of growth by development from the simple form of *Monas* to some of the more complicated animalculæ or Entomostraca. I shall hope in a future communication not only to trace the *Paramœcium* from the monad, but also to show that the "choses extérieures" being altered the monad may become a *Chlorococcus*, an *Oscillatoria*, a *Lyngbya*, a moss, a lichen, an *Amœba*, or a *Mucedo*.

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* Huxley, 'Academy,' Oct 9, 1869.

† 'Quart. Journal of Science,' Oct., 1869.

‡ 'M. M. J.,' July, 1869.

§ Ray Soc., 1853, p. 522.

‖ 'Micrographic Dictionary,' p. 75.

¶ 'Harvey's Manual,' p. 29.

** 'Pop. Sci. Review,' July, 1870.

There may be many other forms which it may be developed to, but for the present I must content myself with the changes in *Paramœcium* to introduce this vast subject fraught with so much importance to our knowledge of the Darwinian hypothesis, the question of epidemic disease, and the formation of the green substratum of Protophyta.

May 20*th*, 1868, in a liquid containing *Oscillatoria* and *Lyngbya* (May 11th) I observed a *Paramœcium* first divide itself into two portions, one globular and the other elongated. They then became attached, and finally the globular was absorbed by the elongated portion. The operation was completed in about three or four minutes (see Figs. 1, 2, 3, 4, 5 and 6).

On *May* 9*th*, 1869, I watched a *Paramœcium* divide, as in Figs. 7 and 8, and then swim away, one globular, the other oval (see Fig. 9).

On the same day I watched two *Paramœcia*, a globular and elongated, become one pyriform body.

Sept. 17*th*, 1869, I saw the Fig. 10 separate into Fig. 11.

May 9*th*, 1869, I watched the spot on Fig. 12 slowly disappear, and the *Paramœcium* become Fig. 13. In further proof that this spot is caused by contraction of the cell wall, I saw on July 25th, 1869, the Fig. 14, in which the contraction at the bottom shows the situation of this spot; but as conclusive evidence I saw the Fig. 15 change its situation, and the contracted spot became visible at a side aspect, as seen in Fig. 16; and on 9th May I saw several green cells protrude amœboid pouches or globules of sarcode from the side, but more especially the *Paramœcium* represented in Fig. 17.* But now to come to the proof of what I desire to show in this paper, namely, that *Vorticella* is only a stage of *Paramœcium*, and that *Callidina elegans* is a developed form of *Vorticella*, I must refer to Figs. 18 to 21.

Sept. 18*th*, 1869, I watched Fig. 18 change into Fig. 19, then throw out cilia at one end, as in Fig. 20, and finally swim away as a perfect *Vorticella*, as Fig. 21, with a telescopic stem.† On the same day I again watched the fissation of a single *Paramœcium*, with two germinal spots, slowly into two perfect globular bodies, neither of them having any spot. At the same time, under the influence of opium, Fig. 22 changed its spot from its position there represented to that indicated in Fig. 23; at the same time the film represented appeared as a perfect membrane, quite transparent outside the cilia—a globule of sarcode.

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* See pl. xxv. (fig. 2*a*), 'Micrographic Dictionary,' by Griffith and Henfrey.

† I have often in previous observations seen the telescopic tail protruded by objects having all the appearance of a detached *Vorticella nebulifera*.

Sept. 22*nd*, 1869, I watched Fig. 24 become Fig. 25, with cilia at each end; subsequently it assumed the appearance at Fig. 26, and finally swam away as a perfect *Paramœcium* as Fig. 27.

July 3*rd*, 1870, I watched Fig. 28 become Fig. 29, and subsequently Fig. 30. In the same liquid I found Figs. 31, 32 and 33, all of which I believe to be modified forms, transitional from some forms of *Paramœcium*, such as *Kolpoda cucullus* to *Vorticella.* During the same observation I saw a *Vorticella* contracting its investing membrane in all directions like an *Amœba*, and a change which gave me much pleasure, a decided *Paramœcium* (Fig. 34) change to an equally decided *Vorticella* (Fig. 35). I also saw Fig. 36 change the end at which the cilia were working and become Fig. 37, and Fig. 38 working cilia at each end successively: also a transparent body resembling a colourless *Euglena* change shape as Figs. 39, 40, 41 and 42, as if to become a *Vorticella.*

July 26*th*, 1870, I found Figs. 43 and 44 among a number of other forms in the pellicle formed on a liquid containing *Vaucheria clavata.*

July 24*th*, 1870, I found a *Vorticella*, apparently intermediate between *Paramœcium* and *Callidina elegans*, closely resembling Fig. 47. But on July 26th I watched Fig. 47 change first to Fig. 48, then to Fig. 49, and finally swim out of the field as Fig. 50. Fig. 51 represents a *Paramœcium*, with a tail like *Vorticella*—this I saw on April 26th, 1868. On Aug. 13th, 1870, I watched Fig. 45, a decided *Paramœcium*, change to Fig. 46, an equally decided *Kerona.*

Some few years ago, during my early observations, before a date was affixed, I made the drawing, Figs. 60, 61, 62, 63 and 64, from *Vorticellæ*, which I had watched.

It now simply remains for me to give my reasons for thinking *Callidina elegans* as merely a developed form of *Paramœcium.*

March 26*th*, I saw Fig. 52, which from former observations I am sure is only the closed form of Figs. 58 and 59.

July 23*rd*, 1870, I saw Fig. 53 change to Fig. 54, both of which I believe to be closed form of the same animal. But on March 26th I saw Fig. 55 unfold itself and become Fig. 56. I watched it assuming all the shapes of *Callidina elegans*, for about five minutes. Fig. 57 is only another shape which the creature assumes, as also Fig. 58. I have made numerous drawings of these creatures, and in some cases I have drawn undoubted *Paramœcia;* but the shape and movements are so similar that I have no doubt that subsequent research will show these various animalculæ and Infusoria to be simply stages of development of one and the same thing, and that is the monad condition which owes its origin to the chlorophyll of vegetables and the germinal matter of animal cells.

The plate which I have attached not only contains no new forms, but the markings are in many cases indefinite; but feeling convinced, as I do, that many if not most of the body-markings are the temporary contraction of sarcode, I have purposely avoided drawing them. Moreover, in such a class of researches as the present, where changes were almost momentary, it was only possible to draw from the best memory, as the lifting the eye from the lens to make the drawing on the spot would have been to forfeit the more important information—the change which might take place whilst engaged in the act of drawing.

It has been chosen to draw attention to this subject of transformation through the agency of *Paramœcium* for several reasons: 1st. It is a common subject of observation, and may be readily obtained. 2nd. It is larger than *Monas*, and the changes are more easily observed and less liable to mistake. 3rd. I have more drawings of changes observed in this than any other infusorium. Moreover, if the principle is allowed, one stage of transformation is as good as another.

It would, no doubt, have been more systematic to trace the subject at once from *Monas* along the line up to *Paramœcium;* but here, until the principle is allowed, much difficulty would have beset our inquiry, which is obviated by the use of *Paramœcium* to endeavour to establish the rule.

In observing on the unity of nature, which seems to be pointed at by this and other facts especially noticeable, while making our investigations on that debatable ground—the margin of Protozoa—we are insensibly led to feel that the orb at least in which we dwell is in an especial manner—

> "Totus teres atque rotundus."

VI.—*Microscopical Examination of Two Minerals.*

By Prof. A. M. Edwards.

There was exhibited, at the last meeting of the Lyceum of Natural History of New York, two specimens of minerals which, on account of their peculiar characters, seeming to indicate that an examination by means of the microscope would reveal facts of interest connected with their source and origin, were referred to me for investigation in that manner. I have viewed them by means of the microscope, and now report upon them as follows:—

The first is a specimen of marble reported to come from a spot on the Adirondack and Lake George Railroad, near Thurman Station, Warren County, and about twenty-five miles from Saratoga in this State, New York, and was polished so as to show its texture very well. In colour it is white, mottled with light green, and having scattered through its mass large patches of transparent, so-called, Noble Serpentine. Mr. G. M. Wilber, who contributed the specimen, was not acquainted with the exact locality, but Prof. D. S. Martin recognized it to be identical with some in his possession, from that portion of New York State indicated.

Without any special preparation and examination, by means of a simple lens alone, the whole mass was seen to consist of *Eozöon Canadense* in a remarkably fine state of preservation. Subsequently I cut slices from it and ground them thin enough to permit sufficient light to pass through, so that the microscope with higher-power lenses could be employed in studying it. Some specimens I acted upon by means of dilute hydrogen chloride (muriatic acid), and compared with very beautiful specimens of the original *Eozöon Canadense* from Canada, and for which I am indebted to my fellow-member, Dr. L. Feuchtwanger. These specimens from both localities, as well as illustrative plates, I exhibited, so that all might see and confirm my discovery, which must be considered as one of considerable importance, when viewed from a geological point of view. It may be observed that the New York specimens are very much finer than the Canadian ones, that is to say, they show the structure of this foraminifer in a strikingly-clear manner, and this the more particularly after the action of the acid. At some future time I may take an opportunity of entering into a consideration of some points connected with the structure and affinities of *Eozöon Canadense*, for in this material thus fortunately brought to light, we have extremely favourable opportunities of studying its intimate anatomy. This I will have a better opportunity of doing when I receive further supplies of the material, which I am endeavouring to procure, and which is said to occur in large quantities at the point from which this was brought.

It is well known that the presence of this fossil is considered to indicate that the rocks containing it belong to the Laurentian group or period, and the bringing of them over the border and down thus far into New York State is of great geological importance. This is the more so, as the members of the Canadian Geological Survey have only lately traced these rocks down into New England, as far, at least, as Salem, Massachusetts.

The other specimen referred to me for examination by means of the microscope, and chemically if found necessary, is the lump of nearly white material exhibited by Mr. J. W. Ward, and thought by him to be finely-pulverized mica, and said to come from a bed of clay in the State of Delaware. At the time of its exhibition I expressed my strong conviction that it consisted of the silicious skeletons of Diatomaceæ, and my suspicions have been confirmed. In fact, strange to say, it proves to be a mass from the now well-known deposit existing at Six-mile Cañon, near Virginia City, in the state of Nevada, and some of which, under the name of "Electro-Silicon" was exhibited by another member at the same time. At this locality this remarkable material is reported to occur in the form of a stratum several hundred feet in thickness. It is rather hard and stony, so it is ground down to a fine powder, and then comes into commerce as a polishing material, and has been rather fancifully christened "Electro-Silicon." It is an example of the kind of deposit I have had already to allude to as Sub-Plutonic, examples of which are so common all through our Pacific States. At some future time I will have something to say concerning the genesis of these deposits.

We have in these two examinations, which I have been enabled to make, further evidence, if it were necessary, which fortunately it is not, of the value of a knowledge of the means of employing the microscope to the geologist; for, using it, facts have been thus readily and in a few minutes settled, which no chemical or other analysis, taking perhaps hours to perform, would indicate.—*From Proceedings of Lyceum of Natural History, New York, U.S.*

PROGRESS OF MICROSCOPICAL SCIENCE.

The Lymphatics of the Lungs.—An investigation into these structures has been recently undertaken in M. Chrzonszczewsky's laboratory at Kiew, by Herr J. Sikorsky. The results have been published in the 'Centralblatt' of December 3rd, 1870, and are abstracted in a recent number of the 'Lancet':—The method adopted consisted in the injection of a watery solution of ammonia into the lungs of living cats and dogs, and the subsequent examination of the paths pursued by the tinted fluid, the lungs being at once removed from the body and frozen, and the blood-vessels being in most cases injected with a solution of gelatine tinted blue. In such experiments it was found that, in opposition to the effect produced in dead animals, neither the intercellular substance, nor the cell formations, not even the columnar epithelium of the bronchia, with which the carmine solution must have been in direct contact, became tinted. The commencement of the lymphatic system was not exactly the same in the bronchia and the alveoli of the lungs. In the bronchia the epithelium generally remained uncoloured; but between the columnar cells were special structures, very similar in form to these cells, and staining strongly with carmine. From these structures minute canals penetrated perpendicularly to the surface of the mucous membrane, and formed a close plexus in the submucosa, and partly also in the mucosa, from which larger trunks originate, which accompany the bronchia to the roots of the lungs. In the alveoli of the lungs, on the other hand, a peculiar plexus, composed of tubes and nodal dilatations, is found; the latter are triangular, stellate, or irregular cavities, which are again connected with the lumen of the alveoli by means of very fine tubules. The lymphatic plexus of the alveoli gives origin to larger vessels, which accompany the veins to the roots of the lungs. This system constitutes the so-called deep plexus of lymphatics of the lungs. The superficial plexus arises from the subpleural alveoli. The trunks pass into the pleura, and then into the pulmonary ligaments.

The Origin and Nature of Fat.—In the 'Transactions of the Vienna Royal Academy,' Herr Toldt has recently published some very interesting observations: In opposition to the statements of Virchow, who maintains that fat-cells are to be regarded as the cells of connective tissue filled with an oily fluid, and are therefore constantly associated with this tissue, Toldt gives as the general results of his inquiries upon the intra-spinal fatty tissue that this, at least, is an organ of a peculiar nature, which neither in regard to its structure nor function can be included amongst the connective tissue formations. In order further to demonstrate that adipose tissue is independent of connective tissue, he refers to the characters and relations of fat in the Batrachia. The masses of fat that surround the uro-genital apparatus of these animals in the larval state consist of large, round, transparent nucleated cells, not separated by *any* intervening substance except blood-vessels. Passing to the Mammalia (says the 'Lancet,' from which we quote), he points out that the first formation of fat in the embryo occurs round the kidneys, and thence gradually extends into the connective tissue of the mesentery after birth. He considers a strong argument in favour of the independency of the adipose tissue to be the fact that it always has, down to its smallest lobules, its own proper and closed system of blood-vessels, which, it is curious to observe, very closely resembles that of the acinous glands. These researches of Toldt enable us to explain the absence of fat in regions where everything appears to favour its formation, as in the sub-muscular connective tissue of the intestinal canal. It explains also the persistence of the tissue, with its characteristic features, even when all the oily matter has been removed by absorption. Whilst fully concurring in the general statement that fat-cells possess in mature adipose tissue a distinct membrane, he differs from Czàjewicz in maintaining that when first formed they are destitute of a membrane, this only becoming visible in the later embryonal periods. The minute masses of protoplasm they contain, however, remain throughout life. He makes an interesting observation to the effect that spring frogs that have fasted through the winter, and are excessively lean, present fat drops in which no membrane is distinguishable, but which, reduced to their protoplasmic primary mass, possess the power of amœboid movements. From the consideration of these facts, M. Toldt has arrived at the conclusion that the protoplasm of the fat-cells, when supplied with sufficient nutriment, is capable, like a gland-cell, of forming fat as a kind of secretion; and, inversely, when the consumption of oxidizable material exceeds the supply, it possesses the power of using up the stored-up fat and discharging it into the blood. The mode in which fat is laid up has also been investigated by Fleischer, with a view of determining whether, in accordance with Liebig's idea, the amylaceous compounds ingested are converted into fat directly; or whether, as Voit thinks, the fat consumed in the economy is derived from the fat of the food, and that the amylaceous compounds are only serviceable as readily combustible compounds, by means of which the fat developed from albuminous compounds, and already present in the body, are preserved. The results of his investigations on cows, which were both numerous and extended over a long period of time, were on the whole unfavourable to Voit's views.

Presence of Fungi in the Ear.—Dr. Karsten has a paper on this subject, accompanied by numerous illustrations, in the 'Bulletin de la Société Impériale' of Moscow (No. I., 1870). The author confirms the statements of Hallier and other previous observers, that when the spores of these parasitic fungi are sown elsewhere, the plants which result from them assume very different forms, according as the substance on which they are sown is rich or poor in material for nutrition; and that fungi described as distinct species, or even as belonging to different genera, are merely different genetic forms of the same plant.

The Ovipositor in Bristle-tails and Spring-tails.—The last number of the 'American Naturalist' contains an extremely able though popular paper on the subject of Bristle-tails and Spring-tails. Mr. L. S. Packard, jun., the author, describes among other structures one which appears novel. He is disposed to consider it an ovipositor. In the genus Achorutes, it may be found in the segment just behind the spring-bearing segment, and situated on the median line of the body. It consists of two squarish valves, from between which project a pair of minute tubercles, or blades, with four rounded teeth on the under side. This pair of infinitesimal saws remind one of the blades of the saw-fly, and he is at a loss what their use can be unless to cut and pierce so as to scoop out a place in which to deposit an egg. It is homologous in situation with the middle pair of blades which compose the ovipositor of higher insects, and if it should prove to be used by the creature in laying its eggs, we should then have with the spring an additional point of resemblance to the Neuroptera and higher insects, and instead of this spring being an important differential character, separating the Thysanura from other insects, it binds them still closer, though still differing greatly in representing only a part of the ovipositor of the higher insects.

Photo-micrographs for the Stereoscope.—Dr. R. H. Ward has been reading an important paper on this subject before the Troy (N. Y.) Scientific Association. Certainly stereoscopic views of microscopic objects ought to be very popular, and would, if properly taken, prove most valuable to the student. Hence we think Mr. Ward's instructions worthy of note. He states that in order to photograph, without delay, any field of view which a working microscopist deems worthy of preservation, he should have a camera mounted on a plank which is blocked at one end for the feet of the stand used as a "working instrument." Then, whenever desired, the eye-piece is removed, the instrument levelled into a horizontal position and placed accurately on the plank, and the magnified image instantly thrown upon the focussing plate of the camera. Finding the usual band, passing around pulleys and over the fine-adjustment wheel, to be a slight annoyance in carrying out this plan with the stand he ordinarily uses (a large stand of the "Jackson" model), he makes the fine-adjustment by a somewhat soft cylinder of india-rubber lying upon the wheel. This cylinder is rather more than three inches long, is an inch and a half in diameter, and weighs about four ounces. It is open through its centre, like a tube with thick walls and small bore, and is mounted upon one end of a straight, light, wooden rod, the other end of which is supported on or near the top of the camera. It is prevented from rolling off from the fine-adjustment wheel by a horizontal wire, transverse to the axis of the apparatus, attached by a hinge-joint to a post at the side of the plank, and to a pin in the end of the wooden rod which just passes through the cylinder; and being retained not over the centre, but somewhat to one side of the wheel, loss of motion is simply impossible, and an extremely fine and manageable motion is secured. The unequalled facility and certainty with which this apparatus can be instantly laid upon the fine-adjustment wheel, or turned back from it, is sufficiently evident.

Approval of Col. Woodward's efforts.—The Medical Society of New York has recently passed a resolution expressing its interest in, and appreciation of, the microscopical work of the United States Army Medical Museum at Washington. The Society approves, with some degree of enthusiasm, the methods of investigation and of disseminating results, employed at the Museum, especially in regard to the study of healthy and diseased tissues; believing that the progress attained is of material use to the profession, and that it would be unattainable at present without the unusual facilities furnished by the Government. We regret that America is so far ahead of this country in this respect, but we cannot avoid thanking Dr. Woodward for the great value of his labours and for the courtesy with which he treats all microscopists in his own country and in this.

Parthenogenesis in the Pupæ of Insects.—There appears on this subject an able paper, by M. O. Von Grimm, in vol. xv., No. 8, of the 'Memoirs of the Academy of St. Petersburgh.' It is a curious instance of Parthenogenesis in a species of the dipterous genus Chironomus. Like the well-known case of Miastor, discovered by Prof. Wagner, this is an example of reproduction by an insect in one of its preparatory, and therefore sexless stages, called *Pædogenesis*, by Von Baer. The formation of the egg-like reproductive bodies commences in the larvæ; but the eggs are not extruded until the insect has passed into the pupa state. It appears that in the spring the larvæ, produced in the ordinary way from eggs, grow rapidly, and after the third change of skin attain their full size and show distinct traces of the pupa within them. The eggs are produced direct from the pupa in this condition. In the autumn the course of development during the preparatory changes is precisely the same; the pupa, however, changes into the imago, which deposits the eggs, probably after copulation, in the ordinary manner. The mode of development of the eggs and ovaries, and that of the embryo in the egg, are described by the author at considerable length, and illustrated by good figures. The eggs are developed in the same way, both in the spring and in the autumn, although in the one case they will be deposited by the pupa, and in the other by the imago; and as they present no difference in their structure, the author regards them all as eggs, and rejects the distinction into *ova* and *pseudova*. He seems inclined to adopt the notion that the supposed cases of Parthenogenesis may be due to self-fecundation.

The Regeneration of Epithelial Formations has been lately investigated by Herr Julius Arnold. He has sought to establish experimentally the source of epithelium in the formation of new skin from granulating wounds. His observations were made upon the cornea, the tongue, and the epidermis (web) of the living frog, and upon the mucous membrane of the hard palate, and upon the scalp of the dog. Arnold failed to confirm the prevailing notions regarding the formation of these cells, by scission of pre-existing epithelial cells, or by genesis from connective-tissue structures. In precisely those points in which the regeneration of epithelium was most active, epithelial cells with multiple nuclei, or in the act of division, were most rare. When the wound was left to itself, the formation of epithelium took place at the periphery, thence advancing toward the centre. Yet, when this was prevented by the removal, in a granulating wound of the hard palate of the dog, of all the peripheral tissue, including the periosteum, the formation of central islets of epithelium was not prevented. The same results were obtained in wounds upon the scalp of the dog.—Virchow's *Archiv*, Vol. XLVII., Part II.

What is the Use of the Spleen?—This question is, we fear, yet unanswered, though Signor Baccelli has attempted its solution. He observes that in the intermissions of the first attacks of malarial fever there is frequently a great increase of appetite, which, however, is soon followed by a gastric catarrh, leading to a very complete loss of digestive power (known even to Celsus) for albuminous compounds. His researches have led him to believe that the increase of appetite at the commencement of the disease is due to simple hyperæmia of the spleen; whilst the disturbance of digestive power, occurring after repeated attacks, was attributable to the persistent hyperæmia of the organ producing physiological disturbance of its functions. The large veins of the spleen, he points out, are destitute of valves, and pass, imbedded in the pancreas, and therefore imbedded in the stomach, and in front of the spinal column, to the liver, so that the blood only traverses them unimpeded when the stomach is empty; whilst, when the stomach is full, the blood current is more or less completely arrested. During digestion, physiological enlargement of the spleen occurs; and, partly owing to the contractility of the organ, and partly to the pressure by the stomach on its veins, its blood is returned by the vasa brevia and coronary veins. But he has found that from the splenic pulp, and from its venous blood, a juice can be obtained containing pepsine, and capable of digesting coagulated albumen. The spleen, he is therefore disposed to think, prepares from the disintegrated albuminates of the blood-corpuscles the pepsine that is afterwards secreted by the glands of the stomach. The hyperæmia of the spleen occurring at the commencement of the malarial intoxication occasions an increased secretory activity of the gastric peptic glands, and thus accounts for the increased appetite observed at this period; but, at a later stage of the disease, the persistence of the hyperæmia causes stasis of the blood in the swollen spleen, accompanied by a kind of paralysis or functional disturbance, and the patient is no longer capable of digesting albuminoid food.

The Range in Time of the Foraminifera.—At a late meeting of the Geologists' Association (March 3rd, 1871) Professor T. Rupert Jones, F.G.S., read a paper on the above subject. The paper (says the 'Geological Magazine' for April) was preceded by an interesting *viva voce* recapitulation of the more noticeable features of the *Porcellanous* (imperforate), the *Hyaline* (perforate), and the *Arenaceous*, Foraminifera, the more important genera of which groups were illustrated by a fine series of diagrams. The variations of form of the Foraminifera are innumerable, and it is extremely difficult to construct satisfactory species and genera. In a strict zoological sense, indeed, a Foraminiferal genus has but the value of a species of a higher class. The tables and lists of genera prepared by the Professor might therefore be compared with lists of species of the higher divisions of the animal kingdom. Of the Porcellanous group, *Miliola*, *Nubicularia*, and *Cornuspira* appear to have the longest range, being found in Triassic and Rhætic strata, and living in our present seas. The Arenaceous are older, five genera occurring in Carboniferous formations. Of the Hyaline Foraminifera, seven genera are Palæozoic. *Fusulina*, *Stromatopora*, and *Eozoon* appear to be essentially Palæozoic forms. The essentially abyssal genera are *Arbulina*, *Globigerina*, *Pullœnia*, and *Sphœroidina*, all Hyaline. The *Globigerina*, as is well known, being abundant both fossil in the Chalk and living in the bed of the Atlantic. Contrary to the general impression, there are very few forms common to the Atlantic ooze and the Chalk, and this leads the author to doubt some conclusions which have recently been drawn.——Professor Morris exhibited some of the Foraminiferal mud from the bottom of the Atlantic, and pointed out the important part the class has played in the formation of the globe, reminding the Association that *Eozoon* forms masses of rock covering a vast area in North America, that Russian Mountain Limestone is made up of *Fusulina*, that the Nummulitic Limestone, of which the Pyramids are built, is found over a very great extent of the earth's surface, and, as Lonsdale was the first to show, our world-famous chalk cliffs are chiefly composed of the remains of this curious group of the animal kingdom. Finally, Prof. Jones explained the method of successive boilings and siftings of obtaining the fossil Foraminifera from clays.

NOTES AND MEMORANDA.

The Library of the late Professor von Grafe is in the possession of Hirschwald, the well-known Berlin publisher and bookseller, who is about to publish a catalogue of it. There are doubtless books on histological matters among the series.

Another District for Eozoon.—At a late meeting of the Lyceum of Nat. Hist. of New York, Mr. G. M. Wilber exhibited a specimen of green and white mottled marble, from near Saratoga, New York, which was recognized by Prof. Edwards, as consisting of *Eozöon Canadense*. The specimen was referred to Prof. Edwards for examination and report.

Fresh-water Diatomaceæ or Infusorial Earths.—Prof. A. M. Edwards recently read a paper on this subject before the New York Lyceum of Natural History. The earths are either of marine origin or lacustrine. Such deposits, says the writer, are extremely common in this country, as well as in Europe, and are generally of a light grey colour, or perfectly white, and extremely light and pulverulent in texture. On the Western coast, through California, Oregon, Washington territory and Nevada, there exist vast tracts covered by fresh-water deposits of Diatomaceæ, which are hard and stony. They have become so by the action of superimposed lava and basalt. Now, it is a remarkable fact that there have come to light two deposits from the Atlantic coast, possessing very much the same physical characters as these Western strata; that is to say, they are hard and stony, and almost white in colour. One of these was presented at a meeting of the Lyceum some time since, by Dr. L. W. Feuchtwanger, but unfortunately its exact locality has not been ascertained. The other is from New Hampshire, and all of the facts connected with its manner of occurrence, doubtless will be brought to light, as Prof. Edwards is at present engaged on the microscopical department of the geological survey of that state, now being prosecuted under the superintendence of Prof. C. H. Hitchcock. A very extensive lacustrine sedimentary deposit of the pulverulent kind, and almost white in tint, has been discovered on the shores of Lake Umbagog, in New Hampshire, and there were indications that it extends beneath the waters of the lake, perhaps over the whole bottom. These deposits are of interest, geologically and microscopically, but at the same time commercially, as they have been used to some extent, not only as polishing powders, but as a source of very finely divided silica, of which the skeletons of the Diatomaceæ they contain are made up, for the manufacture of soluble silicates, or "soluble glass" as it has been called, which is now a considerable article of trade. Although existing for the most part in strata of no very great thickness, or usually covering no more than a few acres of ground, yet, as the number of localities from which they have already been procured in the Atlantic states is over a hundred, the supply for commercial purposes is amply sufficient, without having to draw upon the vast tracts covered by the lake deposits of the Pacific coast, or the marine strata of California and Virginia. In a forthcoming volume of the Report on the Geology of California, the whole subject of these deposits, their mode of formation, with descriptions and figures of the organisms found in them, will be published, when will be cleared up several points of interest with regard to them.

'The History of Zoology,' by Victor Carus, is already in great part printed.

Rocks and Dredgings from the Gulf Stream.—These have recently been chemically examined by Mr. S. P. Sharples. From a microscopic examination of some of the bones, he says that there was little but fibrous structure remaining. They were of a Manatee.

Monochromatic Light.—A correspondent, who does not give his name, and whose letter we therefore cannot insert, inquires what is meant by this term. We really did not think there was any difficulty about the matter. It simply means light of a single colour: thus, red, blue, or yellow light, would be monochromatic. We think Col. Woodward explains the matter sufficiently. At page 153 he says,

" I use ordinarily a solution of the ammonio-sulphate of copper, which gives a bluish-violet light, approximating monochromatism sufficiently for all practical demands." If the rays of light which fall on the mirror of the microscope are previously sent through this solution the whole thing will be achieved. We trust we have rendered this sufficiently clear to an " Amateur."

CORRESPONDENCE.

Portion and Number of the Cilia in Œcistes.

To the Editor of the 'Monthly Microscopical Journal.'

St. James's Mount, Liverpool,
April 15, 1871.

Sir,—Will you allow me to correct Mr. Charles Cubitt's statement, made in the concluding paragraph of his recent paper " On the Winter Habits of the Rotatoria," reported in your last number.

As to the charge that I was " unable to detect the second wreath of cilia on the lobes of the tube-dwellers," it is most singular that he could overlook the following paragraph (page 42 of the Journal for January last) :—

" Mr. Chantrell pointed out a marked characteristic in the *mature* specimens of the *Œcistes crystallinus*, which is the second row of cilia, carrying the food in right and left currents to the gullet or mouth: this does not show in the young specimens, which are always in tubes, but as they get larger the tube gradually disappears."

Further, I exhibited at that time a large drawing I had made of the *Œcistes crystallinus*, contrasting the illustrations of this animal, copied from Pritchard's 'Infusoria,' the 'Microscopical Dictionary,' Slack's 'Marvels of Pond Life,' and from the drawing of *Œcistes intermedias* accompanying Mr. Henry Davis's (F.R.M.S.) paper "On *Œcistes*" (published in the Royal Microscopical Society's 'Transactions'), with my drawings from life of old and young *Œcistes*, showing in Fig. 1 *a well-defined second wreath of cilia*, and *none* in the others. This drawing I have had photographed, and I herewith send you a copy.

Some months ago I sent Mr. Davis several specimens of *Œcistes*, as also of *Floscularia*, from the Windsor pond. We were both clear about the second row of cilia, but differed in opinion as to whether the cilia were inside or outside the disk, I believing the *former*.

Mr. Davis (whom Mr. Cubitt quotes as *an authority* on tube-dwellers) quite agrees with me that there is an *entire absence* of the *transparent* tubes in the *Floscularia* I sent him.

I am, Sir, yours truly,
G. T. Chantrell.

[Mr. Chantrell has sent a series of photographs, which, however, it is quite unnecessary to reproduce.—Ed. 'M. M. J.']

THE 'AMERICAN JOURNAL OF MICROSCOPY.'

44, LAKE STREET, CHICAGO, *March* 20, 1871.

HENRY LAWSON, M.D., F.R.M.S.

Dear Sir,—After some labour I have obtained a copy of the 'American Journal of Microscopy,' published in this city, and noticed in your February issue.

It is only fair to say that it is estimated at its true value by microscopists here, being understood as a mere advertising circular for the introduction of a toy microscope; to this date no second number has appeared.

For a knowledge of its existence our Society is indebted to your notice of it.*

Very truly yours,

CHARLES BIGGS, *Cor. Sec.*

STATE MICROSCOPICAL SOCIETY OF ILLINOIS.

PROCEEDINGS OF SOCIETIES.†

ROYAL MICROSCOPICAL SOCIETY.

KING'S COLLEGE, *April* 5, 1871.

W. Kitchen Parker, Esq., F.R.S., F.Z.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

The Secretary stated in reference to the scientific evening which it had been contemplated should be held on the 26th inst., that the Council had been reminded that this date would be inconvenient, inasmuch as the Linnean Society held their soirée on the same evening, and it was therefore thought better to alter the date of our meeting to the 10th of May next, and a circular notifying the same would be forwarded to each Fellow. Any Fellow who had any object or instrument of novelty or remarkable interest to exhibit, would oblige the Council by communicating the nature of it to the Assistant-Secretary as early as possible before 10th May.

Referring to the omission in the Obituary Notices of the name of the late Mr. Thomas Ross, the Secretary stated that the officers of the Society were in no way to blame, or wanting in respect for the memory of that gentleman. It was by the request of his widow to the Editor that no such notice appeared in the Journal.

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* We have just received the second number of this journal. It is for April. A rather lame excuse is offered for the fact that no number has appeared since the first, published in November. The present is 8vo in size, and is really no better than the first attempt.—ED. 'M. M. J.'

† Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—ED. 'M. M. J.'

The list of donations to the cabinet and library was read, and a vote of thanks given to the respective donors.

The Secretary announced that the Society was indebted to the good offices of Mr. Ladd for a very handsome present from Sir John Sebright, consisting of an Amici microscope and apparatus constructed by Chevalier, of Paris. Along with the microscope was an autograph letter of Amici, with a long description of the instrument in Italian. The instrument would be exhibited at the forthcoming scientific meeting.

A special vote of thanks was passed to Sir John Sebright and Mr. Ladd.

It was also stated that, through Mr. McIntire, Mr. C. Maplestone, of Victoria, had presented the Society with thirty-six slides of palates of the Victorian Mollusca.

The following letter was then read from the Microscopical Society of the State of New York:—

"American Microscopical Society of the City of New York,
64, Madison Avenue, *Feb.* 17, 1871.

"My dear Sir,—It becomes my duty to inform you that at a recent meeting of the American Microscopical Society the following resolution was unanimously passed:—

"Resolved, that the American Microscopical Society have learned with pain of the death of the Rev. J. B. Reade, President of the Royal Microscopical Society of London; and feeling the great loss that microscopical science has sustained by his death, they offer to their fellow-workers in England their sincere regrets and sympathy.

"Yours very truly,

"S. G. Perry, *Cor. Sec.*

"To the President of the Royal Mic. Society,
London, England."

The President announced that Dr. Harkness, of California, was present as a visitor.

The President then read a paper "On the Mode of Working-out the Morphology of the Skull."

Mr. C. Brooke proposed a vote of thanks to the President for his communication.

Dr. Lawson: It was impossible to make any observations on the extremely valuable paper just read by the President. It had laid before the Fellows the results of years of toil, and when he (Dr. Lawson) remembered, in the days when he was a student, how the views of Owen, Melville, and many others who had gone into the cranial theory had been presented to him, he was astonished to see Mr. Parker coming forward with new views, based upon the most elaborate researches. And concerning such views, he felt that he could do just then little more than admire them, and sit down and follow them out, and be content therewith. He considered, as he had said, that it was a most valuable paper which had been read to the meeting, and he was astounded at the immense amount of painstaking research which it displayed.

The President inquired whether any Fellow could inform him which was the best reagent to use in the researches he was making. He had been made acquainted with the value of chromic acid by Mr. Power, of St. Bartholomew's Hospital; but he thought that there were some who used now a solution of bichromate of potash with something else in it, and which, while it produced the same effect as chromic acid, did not render the objects with which they were dealing so brittle as that substance.

Mr. Stevenson suggested that the complement to the solution mentioned by the President was *sulphate of soda.*

Mr. Slack said, that though he could not pretend to discuss the merits of the President's paper on the grounds of comparative anatomy, he wished to make a remark on the allusion made by the President as to the importance of uniting the teleological idea with the morphological. He had paid a good deal of attention to the controversies arising out of that mode of viewing natural objects, and had been strongly impressed with the truth of the statement that there was really no contradiction between the distinct recognition of teleology and that method of science which traced morphological changes from their lowest to their highest types. It was often found that the two schools were in deadly hostility to each other; but tracing, according to the modern principle of morphology, the various steps in the development of an organ did not preclude the belief that such organ was designed for particular use.

The President said that in working out a subject morphologically, the morphological and teleological ideas should be kept perfectly distinct, but the inquirer should be able to give a purely morphological reason for the results obtained, for if he were a true morphologist he had no right to come in with teleology whatever. It would be seen in the order of nature why certain subdivisions had taken place. A remark in reference to the lower part of the cartilage on which the tongue of the sturgeon hung would illustrate this. The sturgeon did not subdivide its hyoid arch in the same manner as osseous fishes, but the subdivisions take place much higher up. He had repeatedly said that the lower portion of the arch was the "symplectic," to which Prof. Huxley had demurred, and his objection had proved to be valid; for when he (the President) had weighed and worked out that subdivision of the hyoid arch in the sturgeon, he had ultimately come by anticipation to that peculiar subdivision of the second post-oral arch which takes place in the frog to form the incus. Here was an explanation of certain morphological changes without going to teleology at all.

Mr. Slack handed in, and described briefly, the contents of a paper he had written "On the Optical Appearances of Cut Lines on Glass."

A paper by Mr. Chas. Cubitt, C.E., was then read, "On Linear Projection considered in its Application to the Delineation of Objects under the Microscope."

Mr. Hogg said the Society was much indebted to Mr. Cubitt for bringing before them the subject of his paper. It was hardly necessary to say that to pursue the subject as he (Mr. Cubitt) had pursued it, required the special training of his profession to carry it on successfully. His labours would certainly tend to correct the imperfect drawings of objects such as he had represented, and which imperfection he (Mr. Hogg) considered to be due in a measure to the objects being viewed under compressoriums. It was nevertheless almost impossible to make accurate drawings of living objects unless the movements of the animal were somewhat restricted. He might refer to the drawings made by Mr. Topping of the fly's tongue, and to the representations of many other objects as an illustration of the effects of compression. So far as his experience had gone, he had always been unable to use the camera lucida for drawing microscopic objects; and having had no training, such as Mr. Cubitt had enjoyed in mathematical drawing, the representations he had made he knew to be often imperfect. He had also found, after repeated trials, that the ingenious instrument devised by Mr. Cooke for making better drawings of living objects was of very little use to him. He thought Mr. Cubitt would admit that not only in the case of *Melicerta ringens*, but also of *Œcistes*, the disk of these very interesting animals, consisting of a hollow sarcode or protoplasmic structure, which was constantly undergoing changes of form, made the work of drawing them very difficult, and accounted for some of the inaccuracies of published drawings.

Mr. Slack said, with reference to the observation as to the change of form which living objects displayed, if the creature, viewed under the microscope, altered its dimensions, the fact should be mentioned, and a front view or side view should be given on the principles of perspective.

A vote of thanks was then given to Mr. Cubitt.

It was announced that at the next meeting a paper would be read from Dr. Maddox, "On the Structure of Lepidopterous Scales, as bearing on the Structure of *Lepidocyrtus curvicollis*;" and also a paper by Mr. B. T. Lowne, "On the Foot of Dytiscus."

The Secretaries announced that some persons had been making use of the late collector's card and name for the purpose of obtaining subscriptions from the Fellows. The act was wholly unauthorized. The only persons appointed to receive money on behalf of the Society were Mr. Reeves, and the Treasurer (Mr. Mestayer); and their receipts alone were valid. This statement was not made for the purpose of casting any reflection whatever upon Mr. Low, as the Secretaries did not believe that he would in any way give his consent to such a proceeding as that which had been referred to.

The meeting was then adjourned to the 3rd May next.

Donations to the Library and Cabinet from March 1st to April 5th, 1871 :—

	From
Land and Water. Weekly	*Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*W. W. R.*
Journal of the London Institution, No. 4	*Institution.*
Popular Science Review, No. 39	*Editor.*
Thirty-six Slides of Palates of Victorian Mollusca, collected and presented by C. Maplestone, Esq., of Williamstown, Australia. Mounted by S. J. McIntire	*C. Maplestone, Esq.*
A Microscope and quantity of Apparatus. By M. Chevalier, of Paris	*Sir John Sebright.*

Francis Alfred Bedwell, M.A., Cantab., was elected a Fellow of the Society.

Walter W. Reeves,
Assist.-Secretary.

Brighton and Sussex Natural History Society.

February 9th. Ordinary Meeting. Mr. F. Merrifield, President, in the chair.—The Secretary of the Eastbourne Natural History Society (the Rev. A. K. Cherrill), and Mr. T. Curties, of 244, High Holborn, London, were elected Honorary Members.

Mr. Wonfor announced the receipt, from the Secretary of the Eastbourne Natural History Society, of copies of papers read at the last two meetings of that Society. The thanks of the Society were passed for the donation.

Mr. T. Hennah exhibited photographs of parallel cylindrical glass rods, rotated over similar parallel rods, to illustrate the effects of illumination. The results were very curious, some exactly resembling the hemispherical markings ascribed to some diatoms, and the "note of admiration" marks of the Podura scale.

Mr. C. Smith then read a paper "On Lichens," in which their nature, the differences between them and Algæ on the one hand, and Fungi on the other; their mode of development, component parts, mode of propagation, their uses and applications, habitats, and other interesting points, were well described; and illustrated by a large collection of specimens, drawings, and microscopic preparations.

A vote of thanks was given to Mr. Smith for his paper.

Mr. H. Goss exhibited a series of British moths whose larvæ feed on different species of lichens, which many of them resemble in colour and markings.

February 23rd. Microscopical Meeting. Mr. T. Hennah, Vice-President, in the chair.—Mr. R. Glaisyer announced the receipt for the cabinet of two slides from Mr. Wonfor; and eighteen slides of vegetable hairs and six illusion photographs, to which reference was made at the last meeting, from Mr. Hennah.

Mr. Hennah, alluding to these photographs of parallel cylindrical rods rotating over other parallel rods, remarked they were intended to show how careful we must be in referring markings to the objects themselves, and not to the mode of illumination, which he believed was the case with some of the markings and "spectral" dots seen and described by Dr. Pigott.

Mr. Wonfor exhibited Beck's illusive photographs of a small tumbler partly covered with hemispheres, and which appeared, according to the way in which the light fell on them, either hemispherical elevations or hexagonal depressions. In illustration of Mr. Hennah's remarks, he would exhibit a slide in which the fibres in plants crossing each other, produced the illusion of hemispherical dots.

Mr. Hennah, inquiring whether any gentleman had any special object to exhibit, said he had brought down some slides prepared by Dr. Addison to illustrate the absorption vessels in plants referred to by Herbert Spencer in the Linnean Society's 'Transactions,' and eighteen slides of vegetable hairs prepared from specimens supplied to him by Mr. D'Alquen.

Mr. C. Smith said he had brought slides of algæ and lichens to illustrate the moniliform gonidiac layer, common to some species of both lichens and algæ, and to which he had referred in his paper "On Lichens."

Mr. Wonfor then called attention to a method of preparing coal sections described in the last number of the 'Quekett Club Journal,' and said he was prepared to show vegetable scales and hairs and fibro-cellular tissue from different species of *Oncidium*, *Arides*, *Pleurothallus*, &c.

The meeting then became a conversazione, when, in addition to the objects mentioned above, Mr. Glaisyer exhibited *raphides* in American aloes, and cuticle of pea *in situ*. Mr. R. Glaisyer exhibited fern scale and sections of coal showing woody tissues.

Mr. Hennah showed a ready manner of making a cell for dry objects with a brass ring and electric cement.

It was determined that the next Microscopical Meeting, March 23rd, would be on "Spores."

March 9th.—Mr. F. Merrifield, President, in the chair.

Messrs. Marshall Hall and T. Francis, jun., were elected ordinary members.

Mr. Wonfor announced the receipt of the January number of the 'Quekett Club Journal' from the Secretary, and of eight reprints of papers on microscopical subjects by Dr. G. C. Wallich, from the author.

Votes of thanks were given to the donors.

The President then read a paper "On Tree Planting in Brighton—Suggested Improvements," in which, after disproving the oft-made assertion that trees would not grow at Brighton, he called attention to many trees and shrubs other than those which had been already tried which might be got to grow to the manifest improvement of the town.

In concluding his paper, he said there was something in the culture of the beautiful, whether in nature or art, which tended to soften the asperities of life and render it more tolerable to those with whom it was a continuous round of toil. The local authorities would deserve well of the town if they enabled those who were confined to a region of smoke and of bricks and mortar, to taste, in the course of their daily avocations, some of those refreshing and pure delights which lead those more happily circumstanced to yearn with an indescribable longing for the "greenwood," and to lavish on the enjoyments of a country residence the hard earnings of the counter or the desk—such delights as the bursting of the leaves and flowers in spring, the sunlight shining through the green luxuriant foliage of summer, or the glorious hues of the woods in autumn, and even the graceful sweep of branches silvered with the frosts of winter.

March 23rd.—Microscopical Meeting. Dr. Dawson in the chair.

Mr. R. Glaisyer announced the receipt of five slides of starches for the cabinet from Mr. Wonfor.

Dr. Dawson, in introducing his subject for the evening—"Spores"—said he considered the spore of a fern was really a seed, and wished the members to pay a little attention to the determining a point not yet sufficiently made out, *viz.* "What a spore really was?" If this were settled it would materially help in unravelling the assumed extraordinary generation of ferns, as held by some authorities.

A seed contained all that was necessary for the development of the future plant, except air and water. With this idea, he boiled powdered asbestos in fuming nitric acid and then washed it carefully with distilled water until all trace of acid was removed. In this he planted mustard seed, which was excluded from the surrounding air by placing it in a closed glass vessel. The seed, it would be observed, had grown and produced cotyledons, from what he called the innate power of growth.

The size of a seed was immaterial; for, however small, it contained within itself material to sustain the plant until it could derive nutriment by the root and plumule. He believed the same held true of the spore, and that it was this active innate principle which gave rise to the prothallus.

As from the axis of the cotyledons the plant grows, so always from one point on the prothallus the fern grows. It would, therefore, be seen that the great point to be determined was the relative position of the spore to the seed. The spore, he conceived, had sufficient innate power to start a prothallus, from which, when formed, the future plant grew, and not by a generation on the surface of the prothallus.

Mr. C. Smith considered the spore simply a cell containing plasma, which formed a chain of cells, from which sprang the future plant.

Mr. Wonfor thought the accepted generation of ferns so contrary to everything else in nature, that it required more than the mere authority of names to be believed, because, by this theory, the sexual organs were described as being developed in the earliest, and not, as in the rest of nature, in the highest state of the individual.

Dr. Dawson wished the members to grow and observe fern spores during the next few weeks, and intimated that he would read a paper in May "On the Fern Spore and Seed Compared."

The meeting then became a conversazione, at which

Mr. Smith exhibited *Ephemerum serratum*, with prothallus and germinating spores of *Pteris serrulata* and *Funaria* twelve and twenty-one days old.

Mr. Ardley exhibited spores of ferns and elaters of Equisetum.

Mr. Davies exhibited specimens of the rare lichen—*Collema dermatinum*, which appears to be a form of *C. fervum;* a fertile specimen of the little-understood *Collema ceranoides*, from West Sussex; *Leptogium Schraderi*, from Woolstonbury Hill; and a fertile *Bryum Donianum*, from near Chichester. This latter plant is seldom found fertile in this country. It is common in Italy, and appears to wander along the shores of the Atlantic, always being found near the coast.

Mr. Wonfor exhibited spores of leaf fungi, ferns and seaweed, spores and elaters of Equisetum and Marchantia, prothallus of fern, and the potato fungus.

It was resolved that the subject for the April Microscopical Meeting should be "Vegetable Cells."

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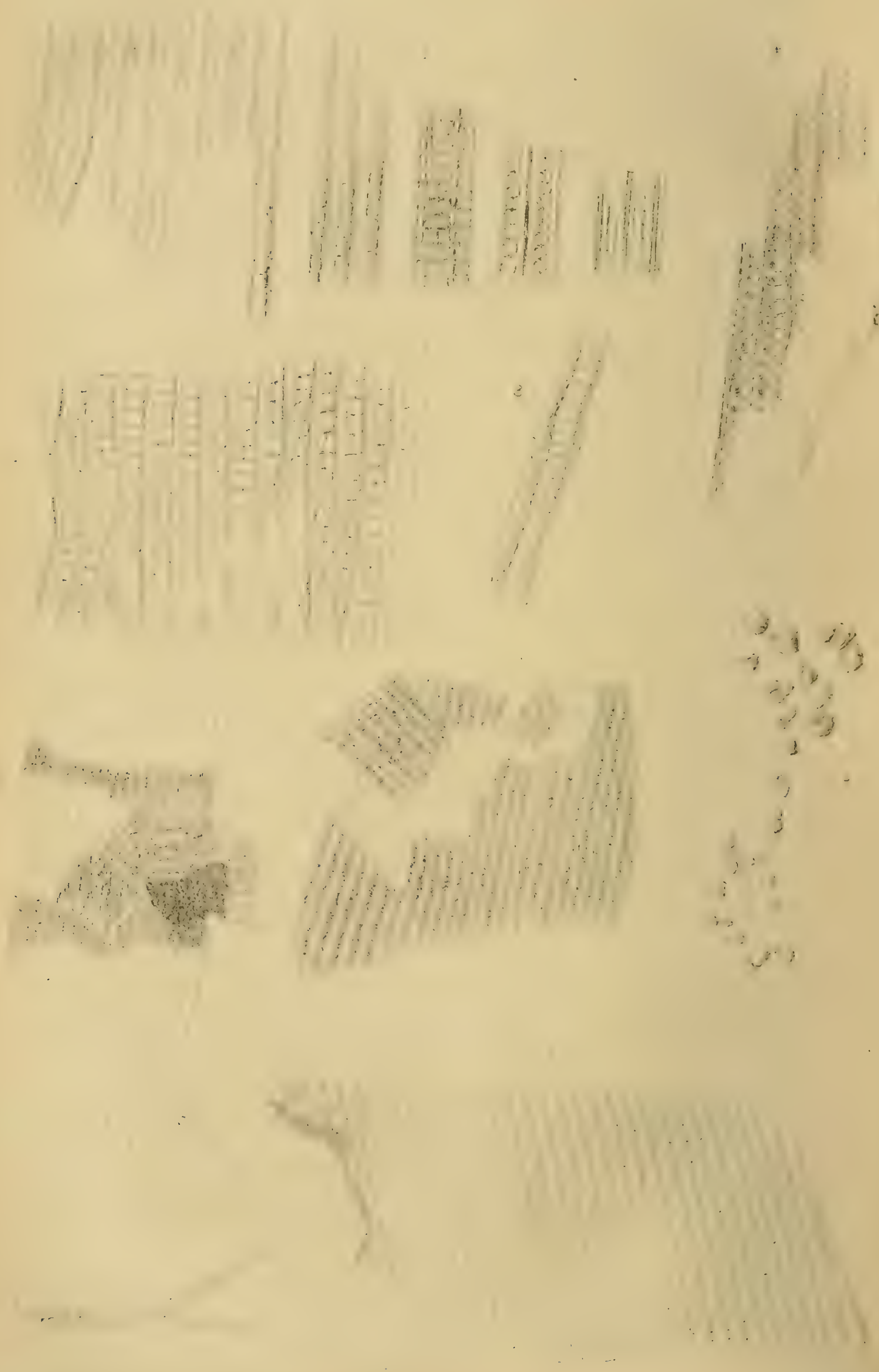

Lepidoptera Scales & fragments.

THE

MONTHLY MICROSCOPICAL JOURNAL.

JUNE 1, 1871.

I.—*Additional Observations concerning the Podura Scale.*

By Dr. J. J. Woodward, U. S. Army.

(*Read before the* Royal Microscopical Society, *May* 3, 1871.)

February 7th, 1871, I had the honour to forward to the Royal Microscopical Society a paper "On the Structure of the Podura Scale, and certain other Test-objects, and of their representation by Photo-micrography." This paper was accompanied by a number of photo-micrographs, one of which (No. X.) was not as satisfactory, in the paper prints, as I could have wished. Subsequent experience has increased my confidence in the opinions expressed in that paper with regard to the scales of the test Podura (*Lepidocyrtus curvicollis*), and a fortunate circumstance has given me the opportunity of representing these views in a more convincing manner.

Shortly after my paper had been forwarded, I received a visit from Mr. Joseph Beck, who is spending a few months in the United States. Mr. Beck had with him an excellent microscope, the workmanship of the well-known firm of which he is a member, and a number of fine high-power objectives by the same house. With these objectives he exhibited to me several slides of the scales of the *Lepidocyrtus curvicollis*, collected by himself from carefully-identified insects. He aimed at the exhibition of the exclamation marks as such, using a right-angled prism, achromatic condenser, and small coal-oil lamp. Before leaving Washington he was so good as to give me one of these slides, indicating on it a scale which appeared particularly adapted to the display of the exclamation marks.

Certainly this scale, which was $\frac{1}{160}$th of an inch in length, and $\frac{1}{700}$th of an inch broad at its widest portion, surpassed in the distinctness of its markings any of the scales of this species which have hitherto come into my possession. With central monochromatic light, the immersion $\frac{1}{16}$th, and amplifier, I obtained a negative showing the exclamation marks better than any representation of the kind I have yet been able to obtain. I send herewith a paper print. The magnifying power is 3200 diameters.

But immediately afterwards, with the same optical combination and magnifying power, without any change in the cover correction, by simply rendering the illuminating pencil oblique, and slightly withdrawing the objective from its first focal position, I obtained a negative which displays the "bead-like" or varicose appearance of the ribbing more satisfactorily than I had previously been able to do. A print of this negative is also forwarded herewith, and I beg that the two may be compared with each other and with those sent with the former paper.

I must add that, since writing the foregoing remarks, I have received from Mr. S. J. McIntire a very interesting letter, in which he explains to me that I was in error in designating the specimen of Degeeria described in my recent paper as *Degeeria Nigro-maculata*. It seems it is really the *Degeeria* or *Seira domestica*. I was misled by Mr. McIntire's label on the slide, which I supposed to be correct. I have never seen either of the insects. This inadvertence, however, does not affect the question of structure.

II.—*Remarks on the General and Particular Construction of the Scales of some of the Lepidoptera, as bearing on the Structure of the "Test Scale" of Lepidocyrtus curvicollis.* By R. L. Maddox, M.D.

(*Read before the* Royal Microscopical Society, *May* 3, 1871.)

The investigation of the intimate structure of some minute objects with which the microscopist is quite familiar, whether for the purpose of demonstrating their integral beauty and exquisite design, or for exhibiting and testing the optician's skill, is a source so replete with difficulty, that the opinions of trustworthy observers are pre-

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EXPLANATION OF PLATES LXXXVI., LXXXVII., and LXXXVIII.

Lepidoptera Scales and Fragments.

Fig. 1.—Part of a China butterfly scale—*Tanonia Œnone*, Ind. China—showing the outer transparent membrane: to the left, a beaded appearance of the framework, out of focus; to the right, various chromatic effects induced by change of focus and direction of the incident light. × 1600.

,, 2.—The same portion of the scale, showing the structural framework of the ribs and transverse bars, × 1600.

,, 3.—Part of the same scale seen reversed, indicating on the ribs what is believed to be the vertical attachments to the outer or middle membrane when three exist, × 1600.

,, 4.—Part of another scale of the same insect, showing the outer membrane and the framework with chromatic interference and pseudo-beading, × 1600.

,, 5.—Part of a large yellowish-white scale from a China butterfly wing—*Papilio Erithonius* (Cram.), Ind. China. At one side is seen the violent chromatic effect of interference caused by the longitudinal ribs and oblique attachments—"striæ"; also the markings of the transverse bars on the (middle or?) outer membrane, for it appears to consist of two layers. × 1000.

,, 6.—An injured dark scale from *P. Erithonius* (Cram.), Ind. China, with a portion of the inner pigmentary membrane turned over, and the middle showing the vertical attachments of the ribs, and the outer membrane, × 1000.

,, 7.—An injured dull-grey scale from the same butterfly, indicating the inner ribbed membrane with its pale, granular, pigmentary deposit, and the oblique framing of the middle or outer layer ("striæ"), × 1000.

,, 8.—Irregular fragments of the external pigmentary deposit from a dark scale of *P. Erithonius*, × 1000.

,, 9.—Part of a scale from the wing of the same butterfly as Fig. 1, showing three membranes—the outer, the middle retaining a part of the framework, and the inner with the heavier portion: also two fragments; outer membrane, *a*; ribs, *b*. × 950.

,, 10.—Fragment of a foreign butterfly scale, received from Mr. McIntire (upper surface of wing and scale), showing the membrane and delicate framework, with indications of a beaded appearance due to thickening at the junctions of the bars and ribs, × 1600.

,, 11.—Fragment of a scale from the same insect, showing a marked pseudo-beaded appearance in the ribs. *The scale is remarkably free of pigment.* × 1600.

,, 12.—Part of a scale from the same butterfly (under surface of wing, upper surface of scale). The transverse bars are rare, except at the edge; the longitudinal ribs, which show the supposed vertical attachments at one or both sides of several, producing when in place the pseudo-beaded appearance, well seen in the rib. × 1600. [Fig. 13.

ferred rather than undertake the examination for ourselves; or else we are disposed to rest satisfied with their general aspects.

Occasionally, however, startled by something overlooked, or by new and imposing views, there is risk of being carried away by the stream of novelty while waiting for extended evidence. Hence

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Fig. 13.—Fragment of a scale of an Indian butterfly—*Terias Hecuba*, Ind. China—slightly charred, showing the outer framework and membrane, × 1600.

" 14.—Beaded chromatic appearance of part of Fig. 17, well charred, × 1600.

" 15.—Part of the same scale as Fig. 18, showing the longitudinal ribs, the transverse bars and the oblique attachments, which give rise to the marked wavy "Test scale" aspect. This figure has been roughly enlarged for showing artificially the production of beads by interference.—*Vide* photographs. × 1600.

" 16.—The direction of the incident light altered, the scale now furnishing the pseudo-beaded appearance at the junctions and interspaces, × 1600.

" 17.—A torrefied scale from the body of the same insect, *T. Hecuba*, showing a marked resemblance to some of the scales of *Lepidocyrtus curvicollis*, × 500.

" 18.—A long scale from the body of the same butterfly, well charred, showing the "notes" and transverse bars ("striæ") simulating the "Test scale," × 500.

" 19.—A body scale from the same insect still more torrefied, throwing the longitudinal ribs into somewhat irregular lines, very much resembling Mr. McIntire's figure, Dr. Woodward's and my own photographs of *Degeeria domestica* (*Seira domestica*, Lubbock), × 500.

" 20.—A scale highly iridescent and of some substance from near the shoulder of the wing of the large China moth (? *Attacus cynthia*), × 230.

" 21.—A portion of the same scale, showing pseudo-beads in the longitudinal ribs and interspaces, × 1000.

" 22.—From the same scale near the tip, showing the framework and membranes, × 1000.

" 23.—A fragment from a similar scale of the large China moth producing two sets of pseudo-beads, one on either side of the long rib, with a dark shadow on one side, × 1000.

" 24.—The same portion of the scale as Fig. 23, showing the framework of longitudinal ribs and transverse bars, × 1000.

" 25.—A fragment of a scale from the wing of the same moth, showing the inner membrane and pigmentary deposit, the transverse bars ("striæ") and (middle or) outer membrane with a portion of the oblique attachments, × 2625.

(On some scales the pigment layer apparently covered the transverse bars.)

" 26.—A coarse scale of *Lepidocyrtus curvicollis*, seen under different aspects.

a, The under surface, first ocus, showing the longitudinal ribs somewhat bent and depressed, marked faintly with transverse markings.

b, The same focussed rather more deeply, producing the beaded appearance of the rib.

c, The same part, the focus carried a little deeper, giving rise to apparently elongated depressions, with the shadows on the same side as in *a*.

d, The same portion under a still deeper focus, producing a dark, cellular arrangement with bright edges.

The illumination remained unchanged in these four figures.

e, The scale, incident light and focus altered from *a*, *b*, *c*, *d*, producing the bent, short, clumsy, pin appearance, slightly marked, and with a greenish shadow on one side, resting apparently on a dark-grey membrane, the points of the pins shading off obliquely. (*Vide* Pl. XXXVII., Fig. 5, of Mr. McIntire's *Templetonia nitida*, 'M. M. J.')

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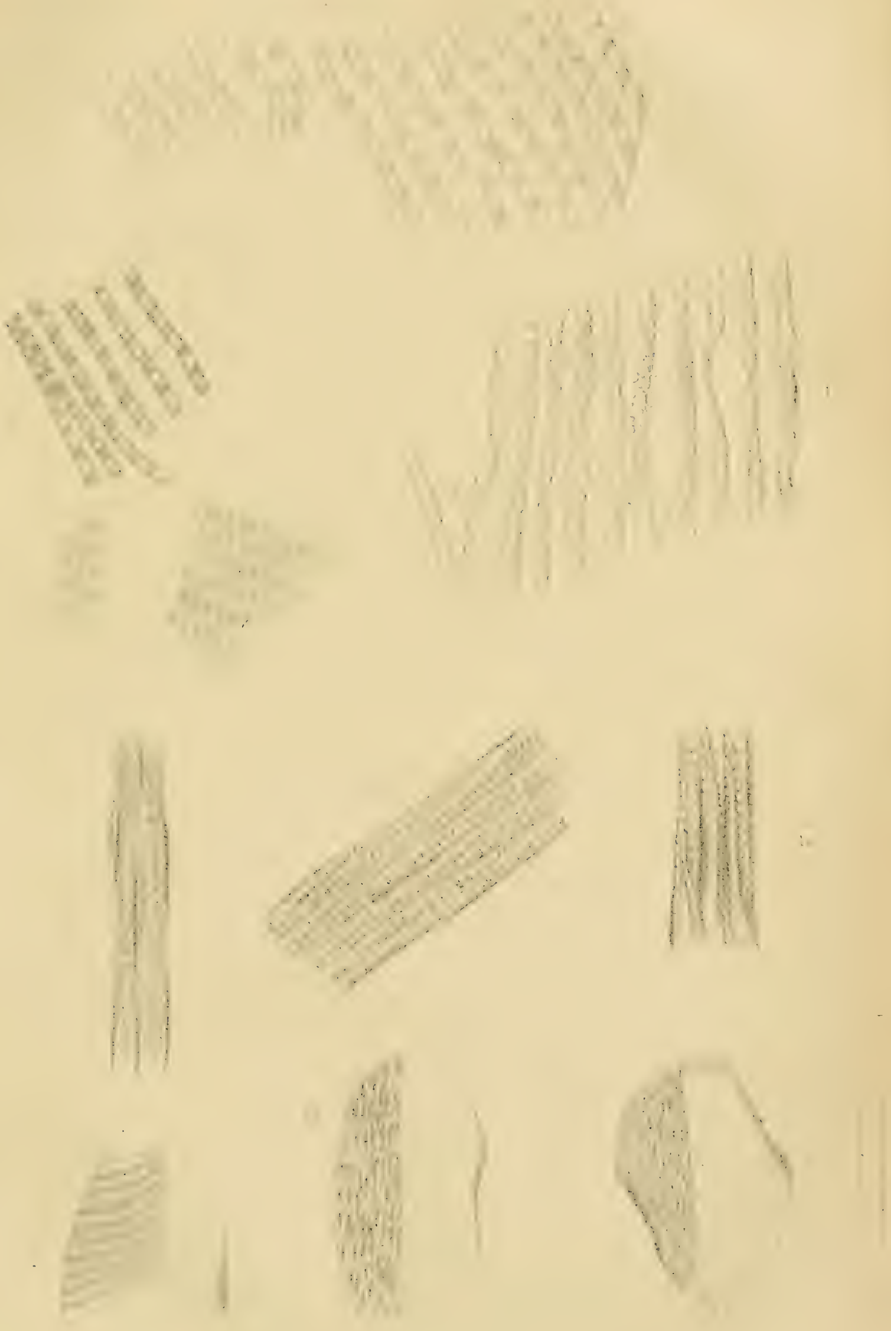

Lepidoptera Scales & fragments.

caution is especially necessary ere embracing new opinions on the structure of minute more or less transparent bodies, however refined may have been the method of investigation, if they be not fully supported by analogy in objects of the same nature, which permit of more easy examination, and certainly it becomes imperative when fresh research tends to opposite conclusions.

By these preliminary remarks it is not intended to convey the idea anyone should remain satisfied with the knowledge already gained, but simply not to be too ready to accept evidence on a scanty basis which for various reasons is open to considerable doubt.

All acknowledge the manifold difficulties that supervene on attempting to decide the structural nature of minute transparent objects, when they are replete with optical effects, resulting from the transmission of light, at varying angles, through irregular surfaces

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f, The focus, only, altered, giving rise to dark, elongated areas—"notes," supported on a pale membrane marked transversely; the heads not rounded, but somewhat acute at one corner, very like the appearance figured in 'Quekett on the Microscope,' plate viii., fig. 4, *a*; and some of the notes in Dr. Woodward's photographs just received.

g, The position of the scale and incident light altered; an appearance approaching the first focus, but with two dark shadows, interrupted, either side of the long rib; the transverse markings still seen.

i, The scale and direction of the light slightly changed, furnishing dark shadows and highly chromatic effects (seen in the coloured drawing), with universal faint striation.

h, The appearance of a framework similar to the Lepidoptera scales, seen with difficulty, and when the light is not thrown sufficiently obliquely to produce colour—all magnified 2625 diam.

The central portion of the figure gives the optician's view, but showing oblique lines faintly.

[The remaining figures refer to some exquisite photographs and figures which have been placed by Dr. Maddox in the Society's Collection.—Ed. 'M. M. J.']

FIG. 27.—A photograph of interference image of Fig. 31, produced by artificial means; simulating beads.

" 28.—A photograph of the same artificial construction, using ¼ plate lenses; simulating *Degeeria domestica* scale.

" 29.—A photograph furnishing bright beaded spots, dark shadow and coalescence of the ribs.

" 30.—A photograph showing faintly to the left (very distinctly in the negative) a chain of dots (? beads) and the general image as a composed or intermediate one between Figs. 28 and 29.

" 31.—The rough pencil sketch of Fig. 15, used to construct the artificial framing.

" 32.—Pencil sketch of injured scale of Humming-bird Sphinx (recent).

a, The edge bars are rubbed into wavy lines.

b, The external coat removed.

c, The little round particles of which the coat consists are rubbed into a confused mass.

d, The surface seen in its natural state; the round particles are most clearly seen on the bars, but they entirely cover the surface.

e, The quill is seen as a hollow tube.

(Fig. and description by Dr. A. Southby, Italy.)

of different thicknesses and refractive powers, or internally stopped, reflected or dispersed. The more the structure retains this characteristic, the greater the difficulty to traverse with confidence the shoals of conjecture.

In venturing to bring before the notice of the Fellows of the Royal Microscopical Society, the subject of the general and particular construction of some of the scales of the Lepidoptera, after the recent and valuable contributions by Mr. McIntire, Mr. Jos. Beck, Mr. Wenham, and Dr. Woodward, as well as the very interesting article by one of your secretaries, Mr. Slack, contributed to the pages of 'The Student,' No. 1, February, 1870, some apology is necessary; but the minute construction of the scales of the Lepidoptera being, so far as I can judge, still open to inquiry, and the subject so full of interest in reference to the views advanced by Dr. Pigott, &c., let me ask your indulgence to be permitted to lay before you the results of a careful, if not very extended, examination, accompanied by coloured drawings of such portions as touch upon the question of beads or beaded structure, as well as the general arrangement of parts in relation to the same, in the "Test scale" of Lepidocyrtus.

If these researches have led me to differ from some authorities in various particulars, the results are stated with diffidence, and with the desire further observations may be undertaken by those far more able than myself to compass the inquiry; but if the evidence can, in any way, tend to place the subject in a more satisfactory condition, or increase the testimony of others, it will amply repay for the trouble and time it has taken. Mr. Slack in the before-mentioned pages of 'The Student' has furnished the opinions of many observers: not to lengthen unnecessarily this article, I shall allude but briefly to them in passing, and refer to his pages for particulars.

The announcement made by Dr. Pigott, in the pages of the Society's Journal for December, 1869, on the structure of the Podura scale, "Test scale," *Lepidocyrtus curvicollis*,* took many of us by surprise—and naturally so, considering this scale to have for years been so continually under observation, and the special study of that accurate observer and artist, the late Richard Beck; therefore to him our thanks are due for rousing us from our wonted apathy, and for pointing out fresh methods for obtaining increased magnification, claiming some advantages over high-power objectives and deep eye-pieces.

To such a contrivance he has given the name "Aplanatic Searcher," and by its means has obtained results which abound with interest; though, to the minds of many observers, they are received with considerable doubt, or referred to optical effects of an illusory nature.

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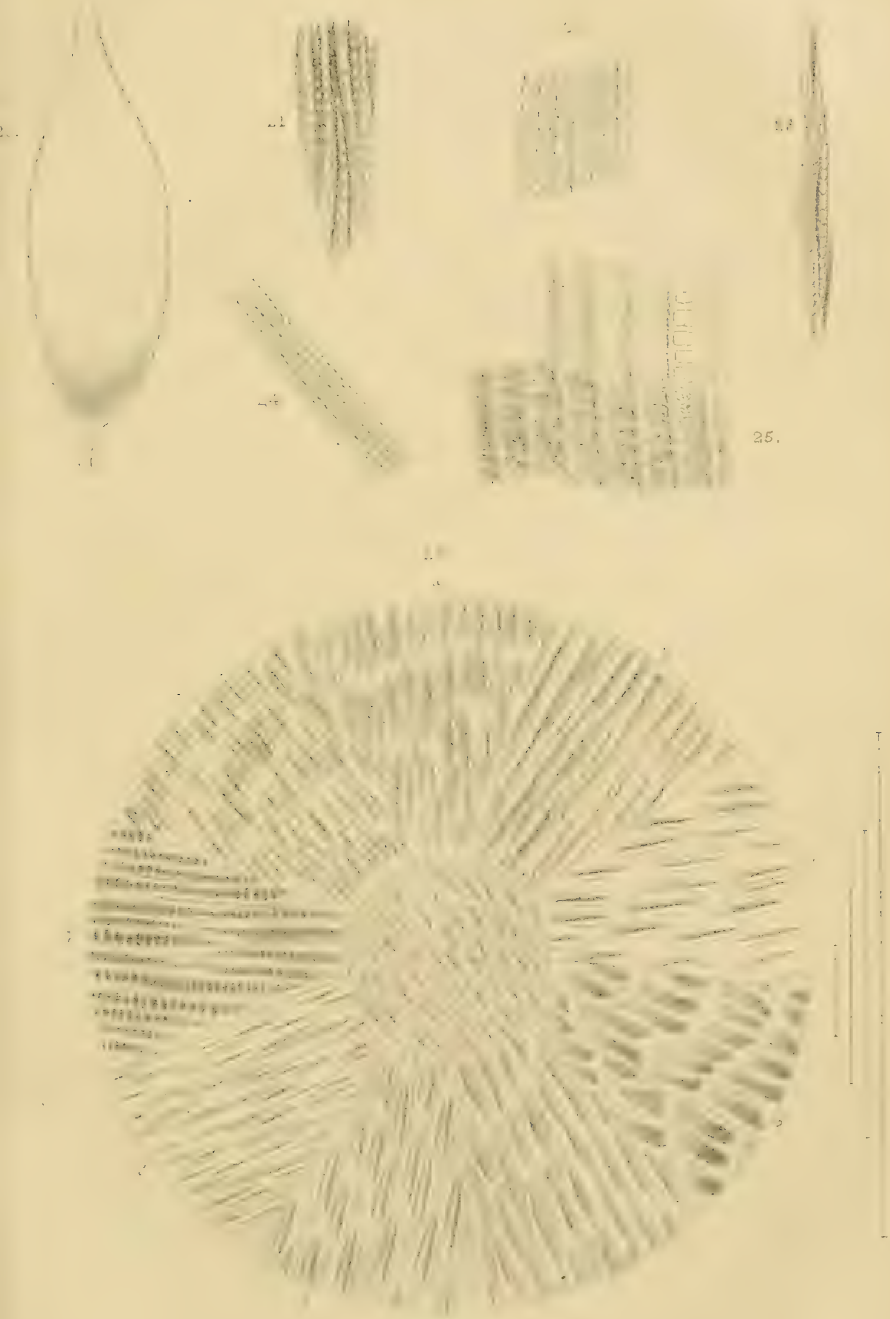
* *Vidè* p. 300.



Lepidoptera Scales & fragments.

Unfortunately, the information regarding the structure of the optical part of the instrument is of such a conservative character, both in the original paper, published in the 'Philosophical Transactions,' and in the 'Journal of the Royal Microscopical Society,' that it does not furnish data sufficient for its perfect construction; and as it appears to have resulted from "error and trial" rather than from computation, hence one reason why others may not be as successful as he appears to have been. I can well understand the tediousness of the research and the patience necessitated to bring such a method to so successful a phase, without losing definition. Doubtless it will stimulate both opticians and amateurs to renewed efforts to cover the "residuary aberration"; but if we do not also get rid of the secondary spectrum, there is something more to be achieved than is yet accomplished. Possibly some refracting cement, as Mr. Grove's purified resin and castor-oil, or Canada balsam and castor-oil, if made one of the components of an achromatic "concave" or "corrector," may furnish some help.

By no means do I wish to detract from the advantages of the "aplanatic searcher" in other hands, but in mine it did not fulfil the expectations entertained. It certainly furnished great facility for bringing out colour in lined objects of different refractive power, and thus perhaps it may have a wide claim to our favour, *provided* we are not carried away by the influence, by reason, as Mr. McIntire so aptly expresses it in his note of Jan. 20, which I take the liberty to quote, that "the colour invests some objects with a new charm." It was not found to increase the defining power of the objective, but this may have been due to want of ability on my part.

It seemed questionable whether three "over-corrected" bodies, compound lenses, eye-pieces, searcher, and objective,* and the additional over-correction furnished by the thin covering glass, would provide, by one traversing between the two others, perfect achromatism.

It may perhaps be as well to point out the error in the original paper, 'Phil. Trans.,' and the appended note in p. 129, where, in the definition of the aberrations, positive and negative aberration have been confounded. The same error is repeated in the present 'Quart. Journal of Micros. Science,' evidently having escaped the notice of the author.

Whatever may be its value as regards depth of focus, &c., of this I feel confident, that at the same time that it furnishes a very ready means for increased amplification, so it also facilitates optical errors in observations. The limitation as relates to light, the length of tube needed, though inconvenient, are quite, if not more than, balanced by the erect image and the use of low powers and eye-pieces instead of high ones. The depth of focus, in many ways, is very valuable, yet when oblique light is employed, *and the object develops colour*, is a constant source of danger to correct interpretation of structure. This, it is believed, will be acknowledged by those accustomed to examine objects structurally.

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* *Vide* editorial remarks on Dr. Pigott's paper in the 'Phil. Trans.,' in the 'M. M. J.,' No. XXVII., p. 129.

When more is learnt of its optical combinations, or some secure method discovered for furnishing a combination as "corrector" and "amplifier," we shall all feel deeply thankful to Dr. Pigott that the employment, long since adopted and rejected by opticians, of intervening lenses, has found a permanent place in the addenda to the ordinary instrument.

The object of this paper being to deal with the general and particular structure of Lepidoptera scales, and their relationship to the structure of the "Test scale," the "aplanatic searcher" would not have been alluded to, had not some of the observations and drawings been made with the form found by "error and trial" convenient, *viz.* a triple and double achromatic, adapted to the proper searcher, racked tube, and employed with a Gundlach's immersion No. 7^{a} ($\frac{1}{16}$th), Wales' $\frac{1}{8}$th, and Smith and Beck's $\frac{1}{5}$th, with a No. 1 eye-piece and the equilateral prism, proposed by your much-esteemed late President, used with a *bright* north daylight, without achromatic condenser, except for the central portion of the Fig. 26 —the object being to simplify as much as possible the mode of observation.

All extraneous light was shut off from the object by a limiting aperture in a thin brass slide, on which the objects between two thin covers, for reversal, were fixed by springs attached to the support; or if the object were on one of the ordinary slides, it was surrounded by an opaque ring or slit, besides other precautions alluded to farther on.

After describing the general appearance with low powers, the waviness of the scale according to the illumination giving place to "*ribbing*, shaded very darkly," &c., Dr. Pigott states,* "with 1200 these ribs have divided themselves into a string of longitudinal beads. But with 2300 they appear to lie in the same plane, and terminate abruptly on the basic membrane. Upon focussing for the strings of beads attached to the lower side, the beading appears in the intercostal spaces." Also, with "more oblique, yet achromatic" illumination, "the intervening spaces showing fine traces of intersecting lines."

Now, if these remarks be understood correctly, the structure of the "Test scale" would be as if composed of a (flat) membrane having on each side rows of beads external to its surfaces; rows of beads longitudinal on the one surface and oblique on the other. Again, "synthetically," the same observer finds proofs of his views in the "finest and most transparent scales of 'azure blue,'" hence we must consider, whatever be the true structure of the "Test scale," the Lepidoptera scale is of the same nature. This admitted, it becomes easy to show that the Lepidoptera scale, though furnishing more or less all the optical appearances of the "Test scale," is in reality not constructed after this method. Thus a doubt is at once raised as to the correctness of the view entertained of the structure of the scale of *Lepidocyrtus curvicollis*, and its congeners.

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* P. 300, No. XII., 1869.

Before touching the microscopical investigation of the Lepidoptera scale, it may be as well to ask if we have in any dermic structure of the animal series one composed of beads lying either side of a basic membrane? I know of none, though of course it does not follow such may not exist; therefore these remarks are open to correction. But what possibly is the function of so elaborate a structure as found in the scales of the Lepidoptera and Thysanuradeæ generally?

Surely little can be founded on such a beaded structure, whether for *strength* in the arrangements, or function, beyond a covering and the power to intensify their iris hues; though if we admit that the generally-received opinion respecting the scale of the Lepidoptera is more correct, as I shall show farther on, then by the substitution of a true framework for beads, we obtain strength, and from its position may eventually find some greater purpose to be effected in the economy, beyond contributing to those wonderful iridescent qualities and common tegumentary relations; and which, possibly, is one effecting a change in their semi-fluids by the action of the external air through delicate investing membranes.

All recent scales that have been examined gave more or less evidence of containing some greasy semi-solid material within the membranes; and possibly it will be found the more the body of the bearer partakes of this fatty nature, the more also the scales participate. Such seems to be the case in the large fatty-bodied moths. This, doubtless, largely facilitates the resistance to wet, which in the case of the Lepidoptera would otherwise be almost fatal to their flight.

In my micro-chemical investigations of some of the common butterfly scales, and which Mr. McIntire did me the honour to insert as a note to his paper "On the Minute Structure of the Scales of certain Insects,"* it was found the contents of the scale could be in part removed by various solvents. In addition to those experiments, to which reference is made, strong sulphuric and nitric acids, &c., have been tried, with the hope of obtaining better characteristics of the internal structure; but perhaps the most satisfactory plan has been to *char* the scales,—a suggestion taken from one of my friend Mr. Wenham's letters, to hand some time since, when speaking of the structure of the "Test scale," and to which he alludes in his remarks on that object in the 'M. M. Journal,' No. XXI., p. 124, but which happy idea he had not, it appears, then carried out, for want of the coarse scales; but I learn from him, April 6, that he has done so since with much success.* This will claim notice farther on.

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* 'M. M. Journal,' No. XXV., p. 3.

Hitherto, with the generality of observers, I had regarded the scales of the Lepidoptera as composed of two distinct membranes with their longitudinal ribs (often showing a very beaded look) and cross-bars; but in addition considered these parts, which have been rather too vaguely defined, formed a framework of union between themselves and the membranes, converting the interspace into more or less minute areas, and hence falling towards Dr. Bowerbank's views, though somewhat differing from them in one or two particulars. He makes them to consist of three distinct layers, *viz.* "the upper membrane, which contains the colouring matter; the striæ, which constitute the frame; the under membrane, which is nearly colourless; and finally, the ribs, which are in reality tubes." Where we are at variance will be seen when the special relations of the parts are considered. It has been only after much trouble I could find a solution to the many appearances, old and new (*i.e.* with aplanatic searcher), in conformity with such arrangement. It was at last to injured, dried and charred specimens more particularly, attention was directed. Without detailing the trouble in attempting to break up scales in particular directions—in fact, to dissect them in various ways, and for convenience—the following remarks will apply to scales of two common characters—though not naturally divided, for some scales partake of both, *viz.* the simple and the wavy scale—derived from their appearance under low powers.

The simple scale, as alluded to by others—as Quekett, De la Rue, Dujardin, Carpenter, Beck, Gosse, Slack, McIntire, &c.—is undoubtedly composed of two or more membranes. The outer, as so beautifully shown by Mr. Jos. Beck's simple experiment,† on Thysanuradeæ scales, and which he kindly made before me, and enabled me to repeat myself, is composed of an almost structureless membrane (Dr. Bowerbank's under membrane?), in most scales exceedingly thin and transparent, in others thicker and coloured (possibly capable of being split into two layers or laminæ, though I have only met with a few examples, and these might have consisted of three distinct membranes), occasionally marked on the inner surface with faint indications of attachments of the opposite surface of the ribs and bars, or of oblique lines, "striæ" of others, and sometimes containing fine pigment granules (Fig. 9).

The inner one, as made up of a coarser and darker material, with also pigmentary deposit, in the substance of which the framework of the longitudinal ribs is fixed, projecting somewhat on the free surface, and in which the minute cross-bars running between the longitudinal ones at right angles are imbedded, projecting on the surface of the membrane facing the opposite one—the "transverse striæ" of others (Fig. 6); but besides this, the longitudinal ribs especially, if not the cross-bars, appear to be attached to the opposing surface of the outer or next membrane—if three, by minute more or less wider or narrower vertical projections or septa (Fig. 12); so that the ribs are bound to each other by cross-bars, and to the opposite membrane by short columns or attachments at very different points; hence, where these draw the two surfaces of the scale so closely together, that they are virtually in contact, a depression at that point occurs in the rib, giving it a lobed, if not twisted appearance, and much increasing the difficulty to separate the membranes from each other. Where they alternate to either side of the long rib, giving it a furrowed figure, and the scale is illuminated in a particular direction, the rib seems irregularly bent, and on either side, a little to the right or left, we see the illusory appearance of a twin row of beads, caused by the vertical partitions or framing *at the junctions*, refracting the light at those points more than at the adjoining; whilst a trifling alteration made in the direction of the illuminating pencil divides the rib itself between those rows, and one or two sharp dark shadows start out on either side of the rib, according to focus, and possibly caused by interference (Figs. 23–26). The transverse bars are most likely also slightly attached by their under surfaces to the opposing membrane where we have indications of a beaded character under ordinary observation; at any rate, the spaces embraced between them are occasionally indicated on the opposite surface under some conditions of the incident light (Fig. 5); or they appear capable of being split (Fig. 9).

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* Now published, *vide* 'M. M. Journal,' No. XXIX., p. 216.

† 'M. M. Journal,' No. XXIII., p. 253.

In the wavy scales the same parts exist; but instead of the vertical attachments being only close on either side of the long rib alternately right and left, and those of the transverse bars, the attachment takes place also obliquely across the scale, from the quill both sides towards the centre, and maps out the interval between the ribs into rhomboidal areas or beads, according to the illumination, and which are under other positions, &c., nevertheless seen crossed by the minute transverse bars belonging to the longitudinal framing (Figs. 15, 16).

When the membranes of the scale so attached can be separated, the lines of junction on the opposing surface of the opposite membrane are easily seen in some, and give the ordinary appearance of faint yet bright lines, or ribs, or bars, resting on that membrane, at varying angles in relation to the length of the scale (Fig. 7);

and where the attachment has also taken place beneath the longitudinal rib, often a corresponding interval is noted, so that the oblique markings appear to follow one another in a series of broken lines: sometimes the lines or streaks seem to pass towards the central rib, as though no union had taken place when crossing the long and transverse bars, though this is rarely the case. The long ribs have a somewhat puckered look at those oblique junctions (Fig. 19). In many scales the strong portions of the framework contract towards each other, or no longer continue a parallel course; and where the rib gets a strongly-twisted look, supposed to be due to the vertical partitions, a strong shadow is cast on one or other side, according to the incident light and position of the scale, the rib itself seeming to shade off towards the oblique attachments. If the transverse bars be continued, as it were, far into the substance of the long rib, they give a markedly beaded arrangement.

It is here, I think, we get a clue to the construction of the "Test scale." For a very fine example of this object, I am greatly indebted to the kindness of Mr. McIntire, and from which slide the drawings on Fig. 26 have been made, in the following order, after carefully studying different scales.

The Lepidocyrtus scale, in the strongly-marked examples, when first focussed upon at its inner surface with high powers, shows according to the illumination a series of ribs as stated by the late Richard Beck, more or less continuous in their length, with faint cross striæ, first directly pointed out, I believe, by Dr. Pigott (Fig. 26, *a*); the ribs seem bent somewhat to one or other side, due it is considered to the vertical attachments, so getting depressed and rather twisted or narrowed as they emerge on either side, towards what is seen to be with other illumination the oblique junctions ("striæ") to the transparent membrane, assuming an outer and inner membrane at least to exist; while these form, as it were, a broken or interrupted septum between them, and thus being repeated at the opposite side of the rib, but at a higher or lower position, we get the boundary of the opposite ends, so as to embrace the heads of what at a little deeper focus forms an elongated, more highly refracting part of the scale, in which the transverse markings, which correspond to the transverse bars of the Lepidoptera scale, are readily seen, and forcibly figured by Dr. Pigott (*vide* Fig. 26, *b*). The fine transverse "striæ" which appear at the first focus, I am inclined to regard as arising from the refractions occasioned at the junctions of the vertical septa, and where these alternate to right and left, they give rise, as in some examples of the Lepidoptera scale (Fig. 23), to a doubly-beaded appearance of the rib, with a dark intervening shadow, seen under another focus and direction of the incident light, as at *i*, Fig. 26.

Still deepening the focus but retaining the same illumination as in *a*, *b*, the refracting portions of the ribs appear like elongated depressions, *c*, having the shadows brought to the same side as in *a*. Again deepening the focus *without any other change*, the elongated depressed areas seem to interrupt the light in every direction, except at their edges and oblique junctions, and thus map out the surface into illusory dark cells with light borders, *d*.

Changing the position of the scale and incident light, we find the rib more highly refracting the light at one end than the other, furnishing the appearance of a clumsily-made bent pin, with faint transverse markings still visible, *e*. The knobbed look is undoubtedly due, in part, to the rib being rather more elevated there than in the other portions of its course, and corresponds to Mr. Wenham's figure,* which is very truthful when an oblique view can be obtained of the scale, and to the knobbed appearance of the notes in Dr. Woodward's magnificent photographs; also in Mr. McIntire's figure of *Templetonia nitida*.†

The interruption of the points of the pins seems to be chiefly due to the depression of the rib at those parts from closer contact with the opposite membrane, causing it also to bend a little off to one side.

Retaining the illumination, but deepening the focus, we produce the figure *f*, still showing the transverse markings, the shape closely corresponding with the figure 4 *a*, × 1250, pl. 7, engraved in Quekett's work on the Microscope, second edition, where it will be noticed the heads of the "notes" are rather notched than rounded, as likewise with very many of the "notes" in the beautiful recent photographs, No. 346, new series, by Dr. Woodward, of the "Test scale," × 3200, received from him the 7th of this month (April), and to whom I feel deeply indebted for the charming picture; also for a stereoscopic picture of the same scale, to hand somewhat later. Although I can easily combine most stereoscopic pictures without the use of any stereoscope, I find it difficult to converge the eyes sufficiently to close these images perfectly, yet when obtained, though the "notes" form somewhat elevated areas above the other surface, they have not the appearance of solidity one would at first sight have expected; but this leads me still more to hold the opinion that their production is an optical effect and not the structural arrangement. The photo-micrographs are wonderful results of skill in focussing the two different views, which scarcely depart perhaps sufficiently from the optician's view,—to furnish a strong stereoscopic image of the structure. The shadows also are more on one side, and the central light is long and narrow.

Still shifting the direction of the scale and the illuminating rays, we produce much the appearance of the first focus, but with two dark shadows on either side of the bright part of the rib; these have been noted by Dr. Pigott (*g*, Fig. 26). The slightest touch of the fine adjustment or prism displaces them, and by a fair appliance of patience, "toying" with the focus and the adjustment of the prism, the image is converted into the appearance at *i*, when the chromatic effects are at their highest; as on either side we have blue and yellow, faint purple and orange, interrupted spaces, crossed by a fine striation, which I believe are the representatives of Dr. Pigott's fine set of beads, if not those brought out on the upper surface in my photographs,* yet caused by the same internal disposition of the parts as in the Lepidoptera waved scales, and giving rise to similar illusory effects. Still, it occurred to me if this were the case, some such structure which is so evident in the Lepidoptera scale as a supporting framework ought to be present. Much time was spent in seeking this, as a friend who seeing me at work naively said, with the head in a bag, as a black velvet covering was so arranged as to shut off all extraneous light from the stage of the microscope, and made to envelop the head; for without such help, and which was constantly used in these examinations, I could not satisfy myself of the reality of Fig. 26, *h*. Fearful prejudice towards this view might influence the interpretation, after being well seen the slide and prism were shifted, and the scale examined at another time with similar success. If the daylight were not very bright, the transverse markings or bars were not seen with sufficient clearness to draw them with the camera lucida, which has been used for all the figures. It is here that monochromatic light, employed after the method of Count Castracane, would, it is deemed, be highly advantageous.

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* 'M. M. Journal,' No. XXI., p. 125.

† Ibid., No. XIII., Plate XXXVII.

These are some of the appearances seen under varying arrangements, and have been made the subject of the Fig. 26, as they are not generally given by others, and may help towards a better understanding of the nature of the scale, and its similarity to the wavy scales of the Lepidoptera. The centre portion of the Fig. 26 represents the optician's view; and here an achromatic condenser was employed, because it brought out the (faintly seen) oblique lines in many parts. This view, so valuable to the optician, I have never regarded, since attempting to photograph the scale long since, as any indication of the real structure; and I should scarcely have dared to figure it, after the beautiful drawings by the late Richard Beck, and the splendid photographs of Dr. Woodward, save, as they are so well known to microscopists, such a figure might, if placed in the centre of the others, tend to the knowledge of the general structure, and facilitate the recognition of the different views given. I may here mention that the scales were examined as opaque objects with the parabolic condenser, but without apparent advantage in any respect.

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* 'M. M. Journal,' No. XX., p. 67, and No. XXIII., p. 261.

It will be gathered from the foregoing and the figures that, like some others (Drs. Pigott and Woodward, &c.), the spines —"notes"!! are not regarded as having any existence as separable bodies, but as due to the refractive nature of the rib at that part, its thickness and solidity, and the direction of the incident light, which opticians know so well how to arrange to the greatest advantage in testing the general "performance" of their objectives—a charming object, doubtless, yet not the whole truth! To quote the words of one of our most skilled observers, the late Richard Beck, when concluding his paper on the photographic appearance of the section of the bossed tumbler, "How can any reasoning be sound when based merely on the appearances of the markings under the microscope, which, as I have endeavoured to show, may be entirely due to the peculiarity of the illumination, and may give not the least indication whatever of the true structure?"*

Nor are Dr. Pigott's views considered as any way indicative of the correct structure of the scale. The beads are regarded as marked optical effects of refraction and interference where the junctions of the longitudinal "ribs" by transverse and vertical septa take place with the opposite membrane, by which we obtain, with the assistance of the oblique attachments, the joint effect of short rods applied to the long ones, and hence illusory, an opinion largely shared by others. In relation to which, I may cite the optical effects procured by crossed glass cylinders, shown so beautifully in Mr. Hennah's photographs,† and which he very kindly sent me. The pseudo-beads are so well represented as real beaded structure, that the appearance would puzzle the most practised eye, and prove how necessary to be guarded in the term beads as components or definite parts of minute structure.‡

Amongst other experiments, an attempt was made, in a rough way, to try and split the scales of the wing of *Pontia brassica.* It was cemented on one side to a glass slide by coloured turpentine cement; then the wing was suddenly torn from the glass, trusting to split some of the scales into thin layers. The scales were left entire. The adherent scales were now subjected to the same violent treatment, by attaching a slide to their exposed surfaces, but from the scale being rendered so transparent by the cement, it was un-certain whether one or two membranes were under view, though evidently some were torn apart; nor was it possible to find with the necessary certainty the corresponding portion of the scale on the opposite glass. Perhaps if tried using only a few scales on a thinly-varnished slide, employing a solution of wax in benzole or chloroform, and covering them with a similarly-prepared stout cover; when separated by force, the scales might be split. This is only a suggestion arising from the previous experiment.

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* 'Intellectual Observer,' vol. vii., p. 95.

† See Report of Proceedings of Societies, M. M. Journ.,' No. XXVIII., p. 195.

‡ Several years since I made photographs of spun-glass hair cut into lengths, then laid on thin glass covers, and attached to them by heat only; next arranged so as to cross or rotate over each other, either with the little rods touching, or with one set resting on the free surface of the other thin cover, or with the free surfaces of both covers in contact. The effects occasioned by interference were highly curious and instructive. Unfortunately, the prints have been mislaid, or they should have been added to the others.

The examinations have been most fruitful on highly-dried or old specimens from hot climates, when anatomized scales were sought. The torrefying alluded to in the previous part has been managed in the following way:—A clean cover had some scales spread out on the centre and examined to see if it contained both the simple and wavy kinds; then covered by a rather smaller thin cover, and both carefully placed on a flat thin piece of iron plate (the iron frame of the large linen-covered buttons answered well). This was held by the forceps at one side, and gradually introduced at its edge into the flame of a spirit-lamp. In a short time the scales were seen to suddenly change colour, and become almost lost to the naked eye. At this moment the support was withdrawn from the flame, but kept at a greater distance from the source of heat, to thoroughly dry the scales. Without the precaution of covering the scales by a second thin glass, they were generally found so contorted as to be useless. Much care is required, or they become fused. The ribs sometimes separated unequally, the interspaces often giving rise to the optical effect of the "notes"!! (*vide* Fig. 18); others shrivelled in various directions, and produced, with some illuminations, most charming chromatic effects; others appeared melted on to the surface, so that a needle could be made to divide them or traverse them in any direction, without displacing the scale. In some, nothing could exceed the beauty of the illusory beaded appearance, both longitudinal and oblique. An attempt has been made to figure the effect (Figs. 13 to 19); and in others the transparent membrane was shown with a portion of the framework attached. On bruising a thoroughly-roasted scale, *there was no evidence whatever of any beads being in or separable from the scales*. The particles of pigment that became detached in every case, most likely from the pigmentary deposit, were very irregular in size and shape (*vide* Fig. 8, from an untorrefied scale).

As regards the nature of the pigment, so called, it is feared the views now offered may not quite correspond with the usually-received opinions. A careful examination with reagents is needed. I have not been able to separate it in quantity sufficient for this purpose, though my friend, Dr. Southby, writes me from Italy, where the Humming-bird Sphinx Moth (*Macroglossa stellatarum*) was plentiful, that he had been enabled to displace it from the surface of the *recent* scale, so as to denude the ribs. Fig. 32 accompanied his remarks of date January 1, 1871.* This would place a pigmentary deposit outside the scale, and this portion I feel disposed to regard either as an exudation of the material within, through the scale membranes, or as the remains of the moisture with which the wings are clothed at the time of emergence from the chrysalis state, rather than as possessing any cellular structure outside the scale. Hence the term here, "pigment cells," seems to be objectionable. Where it existed in a finely granular condition within the substance of the scale, or on the boundaries of the areas produced by the framework, the term "cell" seems less suited; but when in particular spots, or patches of any size, it may seem more applicable, though by transmitted light it becomes most difficult to determine what is due to interference and what to accumulation of coloured material. By the term "pigment cells" we virtually admit their cellular nature; thus it seems open to objection, seeing the term beads is also *sub judice*. Perhaps it would be better to speak of pigment granules where they can be seen as so many separate fine dark or coloured specks in the membranes and edges of the framework of the dark or coloured scales, and pigmentary deposit where a coloured irregular layer can be found in or on the normal free surfaces. Mr. McIntire, in his correspondence, which I take the liberty to quote, suggests "this material never thoroughly dries, and rough usage would modify its appearance": hence perhaps it could be better studied as an opaque object, though a very tedious process with high powers; certainly torrefaction does not much aid in its displacement, and finding this, I hoped more from the use of strong sulphuric and nitric acids, but was disappointed: other reagents may eventually aid in its study, for optically it is feared we shall not arrive at anything very definite. For those scales which are nearly transparent, and where pigment, as such, not necessarily of a dark colour, exists in a highly granular condition, I would suggest that it should in such scales pass by the name of pale granular pigment, or short, pale granules, and where external, pale granular deposit, which would embrace those scales which are of a silvery or pearly-opaque aspect, or which may be of a dead white, varying to any tint of a light colour, even to brick red. This would not bind us to any theory, and give more decision to our studies, yet quite open to improvements.

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* "I enclose you a pencil sketch of part of a scale of the Humming-bird Sphinx. I took the scales with my finger made quite dry from the living insect, then placed them on a clean piece of glass, and took another piece and pressed it upon the first, and gave it a roughish slide on the other; then examined the ill-used scales with my $\frac{1}{5}$th object-glass, corrected for uncovered objects. The scale from which I took the sketch was from the first, therefore the surface seen was the upper surface: you will see that the rough treatment has luckily abraded the cuticle, if we may so call it, on both sides, leaving the internal bars in places quite clean. The under side is the same as the upper as seen in another scale." My views make the outer membrane or surface as the most transparent, and the inner as the chief pigmentary membrane.

It is at the moment of emergence from the chrysalis the study of this substance may be best undertaken, when perhaps the external deposit may be made to adhere to the slide or thin cover. Or the scale from the living insect taken for the examination of the granules within the substance. Possibly it is in the nature of the pigment we may find the function of the scale, if endowed with the property previously suggested.

Whether individually and collectively the pigmented scales may share in the inherent power of "mimicry" described by Bates and Wallace, by which the bearer establishes a close but false resemblance to some of the creatures of its own kind, to avoid personal detection, or to a different order or division of the organic kingdom, as in the simulation of leaves by the leaf butterfly, not only in the healthy condition, but also in "every stage of decay, blotched, mildewed, pierced with holes, and in many cases irregularly covered with powdery black dots, gathered into patches and spots, so closely resembling the various kinds of minute fungi that grow on dead leaves, that it is impossible to avoid thinking at first sight that the butterflies themselves have been attached by real fungi;*—

Or whether, as in the Heliconidæ, any power be shared by them in common with other organs, of furnishing a disagreeable odour, are questions bearing on the subject, but which, for want of specimens, have not been examined;—

Or again, whether the scales harmonizing beauty and utility may not more directly than is generally suspected enter into relationship with the little amatory episodes of these insect creatures, by adding increased brilliancy and splendour to the already highly-attractive nuptial vestments of both sexes;—

Or whether they may be individually or collectively wanting in these endowments, and only modified by the local influences of climate or food, must not be overlooked in their study. Of the variability in *Kallima paralekta,* the under surface is described as so "variable in colour, that out of fifty specimens no two can be found exactly alike, but every one of them will be of some shade of ash or brown or ochre, such as we find among dead, dry, or decaying leaves."

My friend Mr. McIntire, who has given us such valuable particulars of the life and habits of some of the Thysanuradeæ, points out that the change of skin takes place at the time when the scales are in all stages of development, that the growth of the scales is arrested, that they dry up and form a somewhat thick layer of elastic corrugated plates on the delicate skin of the owner; whereas

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* See also Wallace's "Natural Selection," "Malay Archipelago."

I suspect them to retain their freshness and be capable of effecting such changes on the semi-solid material within, as shall keep the scale in its normal state, especially as relates to the pigmented scales in Lepidoptera. Mr. Wenham notices the scales of Lepidocyrtus are limp as wet paper, and certainly it is difficult to unbend them without damage when folded.

I would willingly cede these points, was it not deemed that the pigmented scales require a fuller examination than they have yet received at our hands.

How much of the iridescence of the scale may be due to light reflected from a finely-ribbed surface, or from interference and diffraction in its passage through irregularly-refracting portions, occasioned by the inner framework, or to the fine pigment granules in the substance of the scale, or to the pigmentary granular deposit on the free surface, is doubtful; but we must not overlook the fact that where the pigmentary layer has been accidentally or purposely removed, the chromatic effects are yet very perfect under oblique illumination. It is assumed the pigment material largely determines the general colour of the scale, as yellow, green, brown, blues, reds, &c., of all shades, and the pearly or opaque white by reflected light, perhaps by absorption.

For it was noticed, heat in charring the scale altered its colour to the naked eye, most completely in the very iridescent scales, while under the microscope their structure was not proportionately changed, unless the torrefying had been carried to extremes when parts were often displaced.

How much of the colour may be due to interference or the passage of the light through the object traversing substances capable of affecting the velocities, bending them out of their course, more or less diffracting or breaking them up into prismatic rays, and these again suffering refraction and reflexion in various ways; to at last meet with rays of certain velocities, which either swell their amount of colour, or interfere by want of harmony in their undulations, and either reduce them or modify them, so as to furnish several compound colours in all parts of the object, of more or less corresponding power over the original light, and with interference shadows in addition, is a question open to considerable doubt, if not defying human skill.

And when the phenomena of colour in its complementary characters embrace more than two colours, due in part to the nature of the object and in part to the final corrections given to the objective by the optician, the subject becomes much confused by furnishing a mixture of interference spectra, very charming to the eye, as in Figs. 5 and 26 *i*, but useless in the analysis of structure. Yet if the adjacent parts be not sacrificed in definition, an additional aid may be found for increasing our knowledge of intimate structure, as with polarized light, though, if we disregard the appearances produced by other methods of observation and illumination, which furnish a better clue to the real structure, *without marked chromatic effects*, we shall be in constant danger of accepting illusory appearances.

To the terms "ribs," "cross-bars," at first sight there may appear some objection; but when the figures are examined where the rib and its attachments are seen together stripped from the scale (Fig. 2), the terms seem to me less objectionable than "corrugations" "folds," or "striæ." Now, Mr. Slack sees in the ribs "only corrugations or wrinkles" in the butterfly scales, and I think Mr. McIntire and others prefer these terms to "ribs"; hence it is with diffidence the former are employed, for terms used to convey appearances under the microscope are seldom quite satisfactory. Mr. Slack says, "the beads more or less distinct, or coalescent, as the case may be, I take to be formed by exudations, in drops, from the membranes, consolidating, so far as they do consolidate, in a definite form."* This view appears open to considerable doubt, though a kind of compromise of different theories and contrary to the one adopted in this article, which is perhaps more advanced in particulars than suggested by my friend Dr. Woodward in the recent pages of the 'Monthly Microscopical Journal,' No. XXVIII., p. 158, or than pointed out by that careful observer Mr. McIntire. The inner membrane appears slightly extensible, the ribs can be compressed out of shape in some scales, and in none did they appear to me hollow, as Dr. Bowerbank suggests: a communication through the quill to the inner surfaces is suspected to exist in all scales, and in relation with the membranes of the wing or dermic support.

Whether the butterfly would show any change in the general colouring of the wing, by the transmission of feeble electric shocks through the living insect, is doubtful, though perhaps worth an experiment It will be seen that I am disposed to give to these flattened bladder-like bodies some function, higher than is usually supposed, though the transition from hairs "protective" and "ornamental" is so gradual towards their own varied shape and substance, that they appear more naturally to accord with those ordinary dermic structures. Possibly they may be expended tegumentary organs, which passed their active phase in the chrysalis state, and merely preserved a common attachment upon emerging from this condition. Mr. McIntire points out † the moment they become external appendages they cease to grow; yet does it follow that all changes in the material within the membranes of a semi-solid greasy nature are checked, or are not kept in a normal condition for the benefit of the scale? To some alteration in this material the pigment seems due, and should be referred. It is a point for further investigation and quite open to inquiry.*

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* 'Student,' No. 1, p. 54.

† 'M. M. Journal,' No. XXV., p. 6.

It seemed to me some attempt might be made on a large scale to ascertain whether if two artificial arrangements based on the figures given of the Lepidoptera scale, which also furnished when charred the appearance of the "notes"! ! and of "beads," would, when placed facing each other, furnish an image of beading after the views of Dr. Pigott, when direct sunlight was allowed to traverse them. Hence a large very rude pencil sketch (Fig. 31) was made of Fig. 15; over this was laid a clean piece of glass; the "ribs and cross-bars" were then followed roughly with a composition consisting of mastic dissolved in chloroform and just tinged with aureolin (ground for oil painting), and laid on very thickly in the course of the ribs, lightly over the bars. When dry the plate was reversed, and on this was placed another piece of glass; the ribbings and cross-bars showing through, were then followed in the same manner with a sable pencil charged with a mixture of mastic, wax, and chloroform, untinged and laid on thickly over the ribs, less so over the transverse and oblique bars. When dry these glasses were placed opposite each other at apertures cut in a narrow box, all the light being excluded from beyond the outer edges of the figures, and then held up to sunlight, the image being received on glass dulled (by amber varnish used cold and allowed to chill), placed at variable distances. After a little patience and manœuvring a beautiful dotted or beaded image was shown on the glass, *all trace of ribs and cross-bars had disappeared.* This was several times shown to others, and finally received on a sensitized collodion plate which furnished the negative from which the photograph (Fig. 27) is printed: the lenses were removed from the camera, so that the effect of diffused light through the transparent spaces has given a rather weak image.

The camera was shifted, the lenses inserted (¼ plate, Voightlander) and eight negatives were taken of different appearances; unfortunately in varnishing them the collodion cracked, and too sadly marred the image to be of any use, but the figures of three are added, as also the coarse pencil sketch of Fig. 15. In Fig. 28 a resemblance exists to one view of *Degeeria domestica.* In Fig. 29 several bright-beaded spots will be noticed on the "ribs" and their generally beaded aspect; a dark shadow between the "ribs" and their coalescence.

In Fig. 30, to the left of the figure in the negative are two lines composed of *distinct* small dark spots ("beads"?), but so much more delicate than the rest of the image, that they are almost lost by printing the detail of the other parts, which, before injury by the action of the varnish on an old collodion, gave a medium image between Figs. 28 and 29, partaking of both. Of course the representation was too rude to positively assert that the beaded structure of the insect scale arises from interference and diffraction, &c.; but it has its *value* as bearing on the opinions and figures contained in this imperfect sketch.

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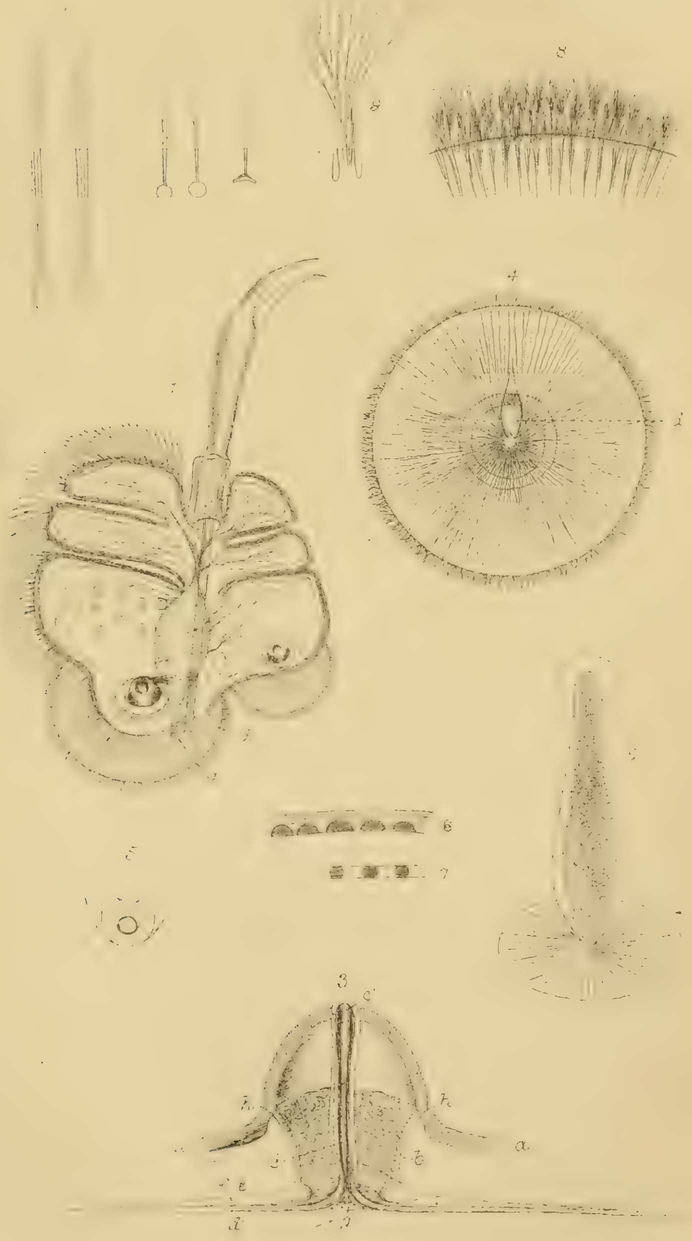
* *Vide* Watson "On Plumules," 'M. M. Journal,' No. VIII., p. 77, and No. XII.

Photography would have been used more largely, had I not learnt from experience that some of the appearances would not have been easy to record without very considerable trouble, especially as the season was not sufficiently advanced to undertake the experiment with either comfort or certainty.

Every care has been taken to render the objects as seen, but to obtain the prismatic effects with all their charm and vigour was hopeless. To quote Mr. Slack's words, "It is only by combining *all* the appearances which seem to give real information, that we can expect to arrive at a true conception of structure;" as aids therefore to others, rather than as dogmatic assertions, it is hoped the foregoing remarks will be received by the Fellows of the Society, to whom I beg to offer every apology for such "dry as dust" details, in venturing to attempt some explanation of the wondrous contrivances, endowed by the Creator with the property that sheds a lustre, both on the gay and gaudy visitor of the hour, or the silent and agile tenant of the dreary cellar.



B.T.L. del. nat

W. West & Co. imp

Foot of Dytiscus marginalis.

III.—*On the so-called Suckers of Dytiscus and the Pulvilli of Insects.* By B. T. Lowne, M.R.C.S.

(*Read before the* Royal Microscopical Society, *May* 3, 1871.)

The anterior pair of feet, in the males of the Genus Dytiscus and its allies, are remarkable for their widely-dilated tarsi. These are well known to microscopists on account of the sucker-like disks with which their under surfaces are furnished. The middle pair of tarsi, in these insects, are also furnished with disks on all their joints, in the males only. The function of the disks in both cases is to assist the male during the act of sexual union. They are entirely absent in the female.

The disks are nothing more than remarkably-developed pulvilli, although their adhesive power is usually attributed to atmospheric pressure; hence they are frequently spoken of as the suckers of Dytiscus.

The simplest form of pulvillus is seen on the tarsus of some plant-loving beetles, and especially amongst weevils. I have selected the beautiful foot-pad of the Australian diamond beetle as an example.

The under side of the tarsus of this insect is densely clothed with a velvet-like pile of golden yellow hairs. Fig. 11 represents two of these hairs seen with a quarter-inch objective and C eyepiece by Ross. They are seen to be tubular, with slightly bulbous extremities. The wall of the bulb is thinner than the rest of the hair. The bases of the hairs are connected with and open into a large gland occupying the interior of the tarsus. This secretes a viscid tenacious fluid which fills the cavity of the hair and exudes through the walls of the bulb, thus enabling the insect to obtain a

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EXPLANATION OF PLATE LXXXIX.

Fig. 1.—The anterior tarsus of a male Dytiscus, with the integument of the upper surface removed to show the gland which secretes a viscid fluid for the supply of the disks. *a*, tendon of claw; *b*, main tracheal vessel and sac. A portion of the sac is removed above to show the openings into the disk-bearing hairs.

" 2.—One of the smaller suckers.

" 3.—Vertical section of the large sucker. *a*, *c′*, *a*, under side of the tarsal joint; *b*, external elastic membrane; *c*, columella; *c′*, opening into gland; *d*, *e*, *f*, membrane of disk; *g*, chitinous cone in the centre of the disk; *h*, *h*, membrane of the gland.

" 4.—Disk of the large sucker; *a*, ovate space without radiating fibres.

" 5.—Internal orifice of the columella of the large sucker seen from the interior of the gland.

" 6.—Transverse section of the disk of the same near the pedicle.

" 7.—Section of the disk near the margin.

" 8.—A portion of the margin of the large sucker.

" 9.—Tuft from the same.

" 10.—Gland hairs from the foot of the blowfly.

" 11.—Gland hairs from the foot of the Australian diamond beetle.

firm hold on the vertical surfaces of trees and the under sides of their branches.

In flies we find an exactly similar arrangement, except that the hairs are extremely minute and delicate, and that they have their bulbous extremities flattened into disks. I have already drawn attention to these structures, and to the manner in which the discoid hairs are detached, in my work on the Fly. I may now repeat, however, that although when all the hairs have the strain put upon them equally they suffice to support the insect, each row may be very readily removed separately. This is done by the manner in which the tarsus is raised. The same seems to be the case in other insects.

In Dytiscus the pulvillus, or sucker as it is sometimes called, consists of much larger and more complicated hairs. I think, however, there can be no doubt it is a mere modification of the usual form of pulvillus, found on the feet of other beetles, flies, and a host of insects.

There are about 200 disk-bearing hairs on the anterior dilated tarsus of *Dytiscus marginatus*, *punctulatus*, or *circumflexus*, the three common British species. Two of the disks on each tarsus are of remarkable size, and differ in several points beside size from the remainder. The largest of the large disks is situated on the posterior portion of the proximal tarsal joint, close to the tibia, and usually measures $\frac{1}{16}$th of an inch in diameter. The smaller is about half this diameter, and is situated in front of the larger. The other disks cover the remainder of the dilated portion of the tarsus, and are not more than about $\frac{1}{150}$th of an inch in diameter. The middle pair of tarsi have their under side clothed with a dense pad of disk-bearing hairs similar to the latter.

If the integument of the upper surface of one of the anterior dilated tarsi be carefully removed, the cavity of the tarsus will be found to be occupied, in great part, by a large but delicate sac. The main trachea with its sacculus and the tendon of the last tarsal joint will be seen lying upon it, and these occupy all the remaining space in the cavity of the tarsus. The sac will be found full of a gelatinous viscid substance, the same as that which exudes from the hairs. It is well supplied with tracheal vessels from the main tracheal trunk, indicating that it is an active secreting gland. If a portion of the upper part of the sac be removed, and its contents cleared away with a camel's-hair pencil, the internal orifices of the disk-bearing hairs of the pulvillus will be seen distinctly in the centre of nipple-shaped projections of the integument which project into the sac. Such a preparation, with a portion of the walls and contents of the sac removed, is represented in Fig. 1.

The largest disk is most easily examined; and a vertical section through its centre is the best mode of showing its structure. Such a one is represented in Fig. 3. It consists of a pedicle, *b*, attached to a dome-like depression on the under side of the tarsal joint, *a*, *c′*, *a*, and a disk, *d*, *e*, *f*. The upper surface of the dome-like depression forms the elevation already alluded to as a nipple-shaped projection into the interior of the tarsus. The secreting sac is closely united to its margin and is represented at *h*, *h* ; its apex is perforated at *c′*.

The pedicle consists of an elastic external membrane, *b*, thickened into a chitinous ring around its junction with the disk, and lined with epithelium. It is hollow, and strengthened by a hollow columella, *c*, which is open along one side of its whole length. The disk is a flattened membranous sac, continuous with the outer membrane of the pedicle above, and in this it differs only in size from the hairs already described from the fly. The disk is strengthened, however, by numerous radiating fibres between its layers, which arise from the columella. This consists of a hollow bundle of chitinous fibres, which are attached above around the opening in the dome, *c′*, and below by a speckled cone of chitine, *g*, to the centre of the disk. The fibres pass over or through this cone in a radiating direction, and become applied closely to the lower membrane of the disk. Here they branch dichotomously, and ultimately unite, at about one-third the diameter of the disk from its centre, with both membranes. They then run straight to the margin of the disk, leaving channels between them, through which the viscid secretion from the tarsal sac passes. This secretion fills the cavity of the hair, which it enters through the columella. The extremities of the radiating fibres on the margin of the disk, invested by a prolongation of its wall, form a dense fringe of brush-like tufts around its circumference. An ovate space on the surface of the disk (Fig. 4, *a*) may be observed destitute or nearly destitute of radiating fibres: it corresponds to the open side of the columella.

I have been unable to discover how the secretion is prevented from flowing from the disks when they are not in use, but I suspect this is effected in the following manner. When the disks are at rest they are cup-shaped; when they are applied to any flat surface they become flattened out. The cup-shaped form of the disk is maintained by the elasticity of the outer wall of the pedicle. This keeps the columella stretched, and I suspect keeps the slit in its side closed. As soon as the disk is flattened the tension is taken off the columella, and I suspect its slit gapes just as a bundle of strings kept together by tension upon their ends separate when held loosely.

The smaller of the large disks is exactly like the larger, except in size. The small disks differ, however, from the large ones in having no columella and no fringe. The pedicle is differently shaped, being pointed at its junction with the disk. The radii of the disk are very delicate, and terminate in the centre of the internal surface of its inferior membrane. The small disks are cup-shaped when at rest, and are not placed at right angles to the pedicle, but obliquely to it. When in use they are flattened and brought at right angles to the pedicle. This opens a communication between the cavity of the disk and that of the pedicle, and so with the secreting sac.

If we compare the structure of the smaller disks with that of an ordinary insect's hair, we shall find they differ only in form. Each consists of an external structureless layer, an internal cellular one, and a cavity. The gland connected with the base of the hairs of the pulvillus is also represented in other hairs. Several caterpillars have a hollow poison gland at the base of certain hairs, which is very similar to the tarsal sac of perfect insects. Both are probably a mere modification of the internal cellular layer of the hair, which is itself only a modification of the internal cellular layer of the integument. For hairs in insects are mere processes of the integument itself. In the larger disks we have the columella added to these structures; but this is clearly only a modification of the cellular layer, which becomes fibrous in all the larger hairs of insects. The differentiation of the cellular layer of a hair into a fibrous and cellular layer, separated by a large space, is a very remarkable modification of the ordinary structure: it is, however, in my opinion nothing more. The structure of the gland in the tarsus is so delicate, and its contents coagulate so readily, that I have been unable to make out whether there is a single locculus for each disk, or whether the cavity of the gland is single. In either case I look upon it as a mere modification of the internal cellular layer of the integument, and identical with the poison glands connected with the hairs of caterpillars already alluded to.

With regard to the functions of these hairs, Mr. Blackwall, in a paper "On the Pulvilli of Insects," as long ago as 1816, showed so completely that most insects climb vertical surfaces or walk on inverted ones by the aid of a viscid fluid exuding from the hairs on the under side of the tarsus, that I feel some apology is needed in reviving the subject. The opinion is, however, very prevalent that the phenomenon is due to atmospheric pressure acting on vacuum suckers. This has been repeated so often by good observers in the case of the fly, and more especially in the case of Dytiscus, that I shall venture to give the evidence we possess upon the subject with regard to the adhesion of the latter. For so far as I know, no one has hitherto denied that this insect adheres by means of atmospheric pressure.

I will now repeat before you an experiment upon Dytiscus first suggested by Mr. Blackwall, and made by him upon the fly, with certain slight modifications made necessary by the nature of the insect. I have here a male Dytiscus rendered insensible by the action of chloroform. I apply its four anterior tarsi to the interior of an air-pump receiver. By pressing these slightly with the side of a needle they adhere firmly to the glass, and the insect remains suspended in its interior. I now exhaust the air, and the insect still remains suspended. It is necessary to render the insect insensible, otherwise it voluntarily detaches itself. To my mind this experiment is quite conclusive.

The evidence afforded by the print of the foot of Dytiscus upon a clean glass slide is very conclusive, as every hair leaves an impression in a viscid semi-fluid material upon the glass. The fringes on the large disks are usually loaded with an abundant supply of this viscid glue.

Again, if the suckers be examined whilst in action, they will be found either to contain air or fluid, or to be applied quite evenly to the surface on which the insect is holding. This may easily be done with an aquarium microscope, or by enclosing the insect in a large live-box, with its feet upwards.

It will probably be argued that a partial vacuum may be formed by the elasticity of the suckers themselves; but, admitting this to be the case, its only effect would be to cause an increased flow of viscid fluid from the surface of the disk, and hence it could not aid the insect in adhering to any surface.

The nature of the adhesive fluid has not yet been ascertained, but it coagulates rapidly, is insoluble in water, and solidified under that fluid. Its surface is slightly greasy, so that water runs off from it, and it cannot be wetted. I have been unable to find any solvent that will dissolve it.

In conclusion, I may remark, that I have several times found beetles which have lost many of the tarsal disks, their places being marked by dark-brown chitinous cicatrices. I think this is due to the insects having become too firmly glued to their mates during copulation, so that they have been unable to escape without leaving their disks behind them. This closely resembles the adhesion of flies to our window-panes in autumn; the tarsi becoming so firmly fixed in enfeebled insects that they are unable to escape.

PROGRESS OF MICROSCOPICAL SCIENCE.

Embryology of Insects.—Mr. A. S. Packard, jun., has furnished the Peabody Academy of Science with a very valuable series of papers on *Diplax*, *Perithemis*, and the Thysanurous genus *Isotoma*, which have just been published. The author enters fully on the development of each insect, and illustrates his observations both by several woodcuts and by a series of most valuable and carefully-drawn plates. He states that in certain mites, as Hydrachna, Pontarachna, Thalassarachna, and probably all the Hydrachnidæ, the ocelli are situated over the second pair of legs at a considerable distance behind the head. In a genus of Annelids (Polyopthalmus) organs of sight are developed on each ring of the body, or, as in certain Planarians, scattered irregularly over the body. In the embryo of Isotoma and Diplax the ocelli are evidently developed on the antennary segment, as they are epithelial cells primarily situated on the cephalic lobes, which seem to form the tergite of the antennary segments. With this view, the supposition he has expressed (led thereto by the generally-received opinion of Milne-Edwards, Dana, and others, that the eyes of insects and Crustacea represent limbs and therefore demand separate segments) seems incorrect. Accordingly, he thinks we are forced to the belief that the head of the hexapodous insects consists of but *four* segments, *i.e.* the second maxillary, first maxillary, and mandibular segments, situated behind the mouth-opening, and the antennary, or first and preoral segment, situated in front of the mouth. The cephalic plates, which fold back upon the head, forming the main expansion of the insectean head, is apparently the tergum of the antennary segment. The clypeus and labrum are apparently differentiated from the cephalic lobes, and thus seem to form a portion, or fold, of the antennary segment. These cephalic lobes in the Arachnids are described and figured by Claparède as folding back on the top of the mandibular and maxillary segments, bearing ocelli and forming the anterior wall of the mouth. The upper part of the head of an insect is, then, largely tergal, rather than pleural as previously stated by him.

Structure of the Bat's Wing.—This, which has long been a mystery, has now been solved in an able paper by Dr. Joseph Schöbl, of Prague. According to him, the bat's wing membrane consists of two sheets of skin, the upper derived from that of the back, the lower from that of the belly. The epidermic and Malpighian layers in each sheet remain separate, whilst the true skin is inseparably fused. In this fused medium layer are imbedded the muscles, nerves, vessels, &c., of the wing. A complicated arrangement of delicate muscles is described, which have their tendons formed of elastic tissue instead of the usual white fibrous tissue. There are also present numerous long elastic bundles stretched in different directions in different regions of the wing. The arteries are each accompanied by a single vein and a nerve, the three keeping company as far as the commencement of the capillary system. With regard to the pulsation in the wing, Dr. Schöbl has nothing new to add to the observations of Wharton Jones and Leydig. The whole wing is covered, both on the upper and under surface, with extremely fine, sparsely-scattered hairs. These hairs are most numerous on the inner third of the hinder part of the wing, and they gradually decrease in number towards the tip. The two wings, taken together, contain from eight thousand to ten thousand of them. They have a general resemblance to those on the body, but are simpler in form. Their length is about 0·2500$^{mm.}$ in *Vesperugo serotinus*, the species principally made use of in these investigations. Each hair sac has from two to seven sebaceous glands, according to the species, and one sweat gland opening into its sac. The two outer fibrous layers of the hair sac have no sharp line of demarcation to separate them from the surrounding connective tissue, but the inner or hyaline coat is highly developed, and, after being constructed beneath the hair bulb, widens out and encloses the sense-bodies (Tastkörperchen), one of which organs is connected with each hair. The nerves of the wing may be considered to consist of five layers, *i.e.* there is one occupying the centre of a transverse section of the wing, which gives off on each side of it four others, and these are successively finer and finer as they approach the opposite surfaces. The inner layer and the one immediately on each side of it, consist of nerve fibres with dark borders, the other layers of pale fibres only. The tastkörperchen are connected with the second layer. The fifth layer of finest fibres ends as a network between the innermost layer of cells of the Malpighian layer of the epidermis. The tastkörperchen are shaped like a fir-cone with a rounded apex turned inwards. They lie immediately below the root of the hair; and their core or central substance is formed of a prolongation of the cells forming the two root sheaths of the hair. Their length is 0·0259 and their breadth 0·0175^{mm}. A nerve containing about six dark-edged fibres is distributed to each körperchen. Just before the nerve reaches this organ it splits into two, and three fibres pass to one side of it, three to the other. The fibres are then wound round the body so as to sheathe its cellular core. Dr. Schöbl thinks it probable that the fibres on one side are continuous with those on the opposite side, and that there is thus a bipolar arrangement here. He attributes to the fine network of pale nerve fibres belonging to the fifth layer the appreciation of temperature, pain, &c.; to the tastkörperchen the highly-exalted sense of touch. It is curious that both kinds of nerve endings are connected with the Malpighian layer of the skin. In conclusion, the author states that he believes he has found similar bodies in peculiarly sensitive places in other mammals, and promises an early account of them.

The Structure of Chitons is being investigated by Dr. P. P. Carpenter, who, we understand, lately communicated a paper upon the subject to the Boston Natural History Society. The paper will be published soon by the Smithsonian Institution.

Microscopy at the American Association.—This Association, which will hold its next meeting on the 16th of August next, will continue its formation of a separate section for microscopy, and we have no doubt that the results will even excel those which we have long since reported from the last session. The 'American Naturalist' suggests to the local committee the propriety of supplying a room with proper light and substantial tables for the occasion. We hope this recommendation will be attended to.

Colossal Fossil Sea-weed.—From the microscopic examination of the structure of specimens of the fossil trunks described under the name of *Prototaxites Logani*, and which Principal Dawson affirmed, in his Bakerian lecture before the Royal Society, to be the oldest known instance of coniferous wood, Mr. Carruthers has (says 'The Academy') discovered that they are really the stems of huge Algæ, belonging to at least more than one genus. They are very gigantic when contrasted with the ordinary Algæ of our existing seas; nevertheless, some approach to them in size is made in the huge and tree-like Lessonias which Dr. Hooker found in the Antarctic seas, and which have stems about twenty feet high, and with a diameter so great that they have been collected by mariners in those regions for fuel, under the belief that they were drift-wood. They are as thick as a man's thigh.

NOTES AND MEMORANDA.

On Bleaching Diatomaceæ, &c.—Dr. R. L. Maddox sends us the following valuable note:—The structure of the silicious valves of the Diatomaceæ, so largely engaging the attention of many microscopists—though seldom before the entire destruction of the endochrome by nitric acid or other agents—I beg to offer for their notice, from some experiments conducted lately in bleaching both marine and fresh-water forms, the use of chlorate of potash and hydrochloric acid as likely to prove *very* useful in the examination, not only of the skeleton, but the disposition of the endochrome; and it seems probable the same may be employed with success on the minute Algæ and minute forms of organic life commonly accompanying them in the collection. The proportions used have been varied, but about 40 grains of the crushed chlorate to 1½ drachm of the acid in 1 ounce of water, placed in a 2 or 3 ounce phial, closed with a waxed cork, may be taken as the average proportions: the action of the evolved chlorine commences after a short period, and can be watched or modified as required. The use of this mixture for the examination of chitinous structures in insects was advocated by Dr. Hicks; but I am not aware of its having been extended to the Diatomaceæ; therefore the suggestion is made in the hope it may be found useful by those engaged in the examination of the organization of these very interesting objects.

Extra Meeting of the Royal Microscopical Society. — We are happy to say that the R. M. S. has determined to give an extra meeting this year in July, and if found convenient, to continue it every year for the future. We hope, therefore, that a good assemblage of Fellows will prove to the Council that they are not mistaken in believing that the vacation is too long at present. The extra meeting this year will be held on the *second* Wednesday instead of the first, owing to the College being unable to spare the rooms on the first Wednesday.

Committee for Testing Objectives. — This, which has been proposed in a letter to us by Dr. Pigott, is favourably noticed in the 'American Naturalist' (May). We think that the writer's suggestion is a good one. It is as follows:—As this subject is largely an international one, though not of sufficient importance to call for the meeting of a committee representing the different countries chiefly interested, any movement, if made at all, in reference to it, should be a concerted movement in England, Germany, France, if practicable, and this country, the same lenses being sent for study from one country to the other. Microscopists might thus be informed, not as to which objectives are the "best," but as to which desirable qualities are possessed in an eminent degree by the lenses of the various makers.

Eyesight and the Microscope. — Under this title there is a short but useful paper in the 'American Naturalist' for May. We merely direct our readers' attention to it, as it is too long for quotation.

Spore-cases in Coals is the title of a very valuable paper by Professor Dawson, in the 'American Journal of Science' (April). He adds to his lectures, delivered in London, and to Professor Huxley's well-known researches, some facts of interest. It is well to point out that this subject was almost first investigated in this country by Professor Morris, to whom the writer gives credit for his researches.

CORRESPONDENCE.

MOUNTING DIATOMS.

To the Editor of the 'Monthly Microscopical Journal.'

SIR,—In the number 24 of the 'Monthly Microscopical Journal' Captain Knight exposes the difficulties which may be encountered by those who wish to avail themselves of the method for selecting and arranging symmetrically diatoms, as exposed by Captain Lang.

If diatoms are rare, I should try butterfly's scales; I may presume to affirm they are very good substitutes as objects, which being equally interesting, permit one to acquire the greatest dexterity. I have been successful beyond the most sanguine expectations, and I work very easily with $\frac{55}{1}$ power (× 55 linear).

But I do not want chloroform, nor benzole, as suggested by Captain Lang; and I dispose the scales, not upon the cover, but upon the slip of glass itself.

As to the Canada balsam, I take great care to use that quantity which will spread under the cover, *and no more.*

Thence no washing or cleaning, which is always a very tedious if not a very dangerous process.

Your obedient servant,

CHEV^R. F. HUYTTENS DE TERBECQ.

BRUSSELS, *April* 22, 1871.

ON CELLS.

To the Editor of the 'Monthly Microscopical Journal.'

ROCHEFORT-SUR-MER, *April* 14, 1871.

SIR,—I read with interest in last December's number of your excellent Journal a note on the method of selecting and mounting *Diatomaceæ*, written by Captain Lang, President of the Reading Microscopical Society. Certain cells made of gold size by Captain Haig, a very skilful micrographer, are described in it. He states that cells of gold size may be exposed to intense and prolonged heat, through the action of which they become perfectly black and carbonized, while the Canada balsam is unable to alter them. The process which he describes for calcining such cells requires a great deal of time and some trouble, and I see no need for its adoption. Excellent cells for microscopical preparations may be made of *Bitume de Judée* (which is naturally black), of asphalte, or of marine glue dissolved in spirit of turpentine or in benzine. If the smoke, or rather the vapour, disengaged from gold size, already calcined, and heated again, is apt to form crystals, much more strongly must this effect take place when bitumen, not previously heated to excess, is used. In order to remedy this inconvenience, I have been accustomed to prepare beforehand a number of cells, and so to have always at hand some dozens of slides ready for use; but at the moment of using them I do not expose them to heat. Formerly, in order to make the cover adhere, I used to apply a second layer of varnish over the first. This sometimes caused an inconvenience more serious than the one already pointed out. The new bitumen spread itself gradually into the preparation, and at last altered it completely. There is a very simple means of avoiding this danger, and I will now describe it. A circle of bitumen, about one-third smaller than the covering glass, is drawn beforehand on my slides. When I wish to make a preparation, instead of coating, as formerly, the first circle with a second layer of bitumen, I form a second circle of it outside the first, and as near as possible to it, and each of the two circles has its own advantage; the first, in fact, while forming the cell, serves as a support for the covering glass, and thus preserves the Diatomaceæ from any breakage; it offers besides a serious obstacle to the spreading of the more liquid bitumen of which the outside circle is composed, and the latter closes the cell by fixing the cover, which, when the preparation is dry, may be covered with a final circle of bitumen. It is of course understood that I am speaking of preparations made in the dry way only, and not with balsam.

I hope this communication may be useful to your readers, and enable them to escape many a failure.

I remain, Sir, yours,
MOUCHET, H.F.R.M.S.

AN ERROR.

To the Editor of the 'Monthly Microscopical Journal.'

MUSEUM OF COMPARATIVE ZOOLOGY,
CAMBRIDGE, MASS., *April* 10, 1871.

DEAR SIR,—In my letter to Mr. Charles Stodder, published in your March number, there is a misprint. In the fifth line, the word "special" should be "spectral"; it should read, "with no spectral lines." By making the above correction you will oblige

Yours truly,
E. BICKNELL.

PROCEEDINGS OF SOCIETIES.*

ROYAL MICROSCOPICAL SOCIETY.

KING'S COLLEGE, *May* 3, 1871.

Dr. Braithwaite, F.L.S., in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations to the Society was read, and a vote of thanks given to the respective donors.

It was announced that the Council had been advised that it would be convenient to many of the Fellows to have one more meeting during the session than it had been the custom to hold. The session had usually terminated in June; but as many Fellows would be in town in July, the Council had thought it practicable to hold a further meeting, subject to permission from the College authorities, on the *second* Wednesday of July, the College being occupied on the *first* Wednesday, which otherwise would have been chosen. If the experiment should succeed, and a desire were expressed for its repetition, the Council would take the bye-law relating to the subject into consideration, with a view to its alteration, in order that the session may close in July instead of June.

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* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—ED. 'M. M. J.'

Mr. Ladd described an addition he had made to the microscope, which he had found very useful in examining double axial crystals with polarized light. Generally speaking of the apparatus employed, the polariscope was a very expensive instrument. The little apparatus he had devised could be applied to any microscope with very little trouble and cost. In the construction of it no new principle was involved, although the application was new. Under the stage of the microscope is placed a deep lens, beneath this another lens not quite so deep, the ordinary Nichols' prism being added in the usual manner. Above the stage this arrangement of lenses is repeated. The draw-tube of the microscope must have another lens; it is useful to focus the object of the draw-tube. The eye-piece used was the ordinary A eye-piece, with the Nichols' prism over it in the usual way. The advantage of having these lenses was that a wider range of field was obtained, for by the arrangement described the two axes of sugar could be brought into the field; and it was well known that it required a large view to take in those two axes. The arrangement works better by daylight than by lamplight. He had seen another instrument that day at Mr. Powell's, but it would not be possible with that to bring in the two axes of sugar.

The following note was read from Mr. Suffolk:—

"CLEREMONT LODGE, PARK STREET, CAMBERWELL,
April 27, 1871.

"GENTLEMEN,—I forward herewith preparations of vegetable fibres, and chiefly from specimens supplied by Dr. John Forbes Watson, Reporter on the Produce of India, and by whose kind permission I am enabled to present the accompanying specimens. To these I have added one or two received from Kew, and two slides of recent fibres grown by myself.

"The chief value of the whole is as a trade collection rather than as a botanical one; they are arranged as the duplicate series is in my own cabinet.—Manufactured materials, not only including spun and woven fabrics, but also fibres obtained therefrom.

"I hope during the winter session to give the Society a paper in explanation.

"Yours truly,

"W. T. SUFFOLK.

"TO THE SECRETARIES OF THE ROYAL
MICROSCOPICAL SOCIETY."

Dr. Braithwaite said, that as the number of fibres produced by the vegetable kingdom useful for textile fabrics was increasing every year, it was a very great advantage to have the actual notion of the nature of the fibres produced by certain plants as objects of comparison when working in that department. He proposed a vote of thanks to Mr. Suffolk, which was unanimously passed.

A paper by Dr. Maddox, "On the Structure of Lepidopterous Scales, as bearing on the Structure of *Lepidocyrtus curvicollis*," was then read.

Mr. Slack said, if Dr. Maddox had found the pigment in the scales disposed in the form of beads, the appearances were a confirmation of his own views on the matter of dispute.

Mr. Hogg said the effects produced by Dr. Maddox were obtained while the scales were in a torrefied condition. He (Mr. Hogg) had also made experiments on the torrefied scales in the way proposed by Mr. Wenham. The pigment seemed to run up the longitudinal markings, giving a continuous effect, and under the $\frac{1}{18}$th immersion lens, which showed them remarkably well, the notes of exclamation ! ! were quite destroyed. No doubt the structural characteristics of scales can be better studied in the larger species of the Lepidoptera, as Dr. Maddox has pointed out in his valuable paper, when it is found that they are not inserted "as tiles are in the roof of a house," but are as much of an outgrowth of the dermal structures as are the hairs of animals or the feathers of birds. Their bulbous extremities, being firmly imbedded in the membranous wing, and filled with an albuminous or fatty colouring matter, this flows up the shaft, permeates the tubules, or series of longitudinal folds, for the purpose of nourishing the scales, in precisely the same manner as the epidermal structures covering animal bodies derive nourishment and are maintained in health. These closely-placed tubules filled with colouring matter give the iridescent hues and colour so much admired in the wings of Lepidoptera: as soon as death takes place the pigment shrivels up, leaving certain interspaces, which, when viewed above transverse or irregular markings in the basement membrane, heightened by an oblique pencil of light, produce false appearances of "rows of beads." The beautiful drawings sent with Dr. Maddox's paper show this to be the explanation of the structure of scales, while his photographs, and also those of Dr. Woodward, prove that pseudo-beads can as easily be produced by artificial means.

Mr. Slack said, that Mr. Hogg's experiments on Lepidopterous scales showed that their structure was analogous to that of the feathers of birds. He had noticed the bulbs described by Mr. Hogg, and he apprehended that the pigment matter was either conveyed through the quill or by exudation from the membrane; and as it shrunk it tended to divide into portions more or less beaded. Mr. Hogg's experiments seemed to him to lead to conclusions which he (Mr. Slack) had previously ventured to bring before the Society.

A short communication was also read from Dr. Woodward, entitled, "Additional Observations concerning the Podura Scale."

Votes of thanks to Drs. Maddox and Woodward were then given.

Mr. Lowne read a paper "On the so-called Suckers of Dytiscus and the Pulvilli of Insects."

Dr. Braithwaite said, the sucker of the Dytiscus was one of the stock objects of a microscopical cabinet, and the functions it had been supposed to perform had been copied from book to book, without any test being applied as to the correctness of the descriptions. He had no doubt that Mr. Lowne had that evening explained the true functions, and he believed the explanation would carry conviction to the minds of all who had heard it. He proposed that a vote of thanks should be given to Mr. Lowne.

Dr. Lawson inquired whether Mr. Lowne had found these suckers on the female as well as the male insect.

Mr. Lowne said he had found them only on the male, and that they were chiefly used for purposes of copulation. Sometimes the glue they contained became so hard by drying during copulation that the suckers were left behind by the insects, and in other cases cicatrices were produced by the hardened glue.

Mr. Slack thought that Mr. Lowne's hydrostatics should be accepted with some limitation; and he explained that it was theoretically possible to have a film of water so thin that the capillary attraction between two pieces of glass would completely counteract the atmospheric pressure.

Mr. Lowne said, the principle he contended for was that two bodies would be kept together under the conditions mentioned by him by the attraction of cohesion and not of atmospheric pressure.

A vote of thanks was then given to Mr. Lowne; and the meeting adjourned until the 7th of June.

Scientific Evening.

On the 10th May the Society held a scientific meeting for the purpose of examining new or remarkable objects and apparatus, and conversing thereupon. To avoid crowding, the admission was restricted to Fellows, and to a very few visitors contributing to the exhibition. About 150 persons attended, and from the subjoined list of the principal objects displayed, it will be seen that matters of great scientific interest were brought forward, and those present were much gratified by the opportunity of viewing them, and obtaining information as to the latest results. It was especially satisfactory to find the arrangements thoroughly appreciated by Fellows residing in the country, several of whom came from great distances.

The Society exhibited two of the most recent instrumental donations,—a very curious reflecting microscope by Cuthbertson, the gift of Dr. Miller, and an elaborate microscope by Chevallier, on Amici's plan, presented by Sir J. Sebright.

Mr. Stephenson's new binocular, in two forms, one having the stage horizontal and the tubes sloping at an angle convenient to the observer, and the other having the prisms inserted in the cell of the objective and fitted to an ordinary instrument by an adapter, attracted special attention, as did the immersion lenses by English and Continental makers.

Mr. J. W. Stephenson: The new binocular microscope, with safety stage and $\frac{1}{8}$th objective; Stephenson's erecting binocular 2-inch object-glass, protected by a glass tube, forming a cap with thin glass end plunged in sea water. Living Terebella and Balanus.

Dr. Millar: A slide of very rare Polycystinæ under Stephenson's erecting binocular microscope.

Mr. S. J. McIntire: *Cheyletus eruditus, alive;* Podura scale, *Seira Buskii.*

Mr. Delferier: *Nav. rhomboides,* &c., with Powell and Lealand's new immersion $\frac{1}{8}$th.

Mr. Curties: Podura scale illuminated as an opaque object by Wenham's truncated lens and paraboloid, with a $\frac{1}{32}$ objective of Gundlach.*

Dr. Braithwaite: Marchantia-like prothallium of *Sphagnum cymbifolium*, showing how it differs from the true mosses.

Dr. Gray: Section of horse's hoof, showing the papillæ.

Mr. G. H. Makins: Crystallized gold, silver, &c.

Mr. E. Richards: Apparatus for the more scientific working with Darker's selenites.

Mr. E. Wheeler: *Aulocodiscus Kittonii*, &c.

Messrs. Powell and Lealand: *Amphipleura pellucida* and *N. rhomboides*, shown with new $\frac{1}{16}$th inch objectives; and Podura scale shown under a $\frac{1}{8}$th inch with a new adjustment for correcting the colour with different thicknesses of covering glass.

Mr. Hailes: Some slides of Foraminifera, from the 'Norna' expedition.

Mr. Fielding exhibited for Mr. Marshall Hall a fine specimen of the new sponge, *Pheronema Grayii*, and slides of the spicules.

Messrs. Ross: Some beautiful injected preparations, and their new immersion glasses.

Mr. Walter W. Reeves: 'Glochidium,' the embryo state of *Anodonta Cygnea*, the large fresh-water mussel.

Mr. Blankley: New portable microscope, with fine adjustment, and sufficient rack for a 5-inch object-glass.

Mr. How: A new lamp.

Mr. Norman: A portable cabinet to hold 141 slides in the space of 7 inches by 5 inches.

Mr. Browning exhibited a series of diffraction spectra of magnesium, aluminium, and other metals, the metals being burnt by a powerful induction coil, with a new spark condenser, the grating being a series of fine lines, 6480 to the English inch, exquisitely ruled by Mr. Lewis Rutherford, of New York.

Mr. Browning also exhibited one of his improved micro-spectroscopes, with bright line micrometer, and showed in this instrument the absorption bands in the spectra of the following substances,—chalcolite, salt of didymium, uranus oxide, &c., &c.

Dr. Pigott exhibited the false images and distortions of a gas flame produced by what has been considered a fine $\frac{1}{4}$th objective, and at a later period *Pleurosigma formosum*, seen with a $\frac{1}{2}$ inch by Wray, brought by his aplanatic searcher to a power of 1200 linear, with excellent definition. It was found impossible to show this properly while the floor was in vibration from persons walking in the vicinity.

Mr. Fitzgerald: Crystals of silver formed by electric decomposition of a silver salt under the microscope.

Mr. Ladd: His new arrangement, adapted to ordinary microscopes, for displaying the polarizing effects of double axial crystals, the field wide enough to take in both axes of sugar.

Mr. Slack: A series of crystals, sulphate of copper, salicine, hippuric acid, chlorate of potash, &c., obtained from solutions containing colloid silica, which modifies the ordinary forms, and affords fine effects with the polariscope.

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* See 'M. M. J.,' No. VII., p. 25.

Messrs. Beck and Beck: The Podura scale, shown under their new $\frac{1}{10}$th immersion object-glass. It was found that by a very slight change of focus one of Dr. Pigott's series of beads was clearly displayed.

Mr. Suffolk: Splinter of lucifer match, $\frac{4}{10}$ths objective. Parabolic Lieberkühn.

Mr. C. J. Fox: A figure produced by grinding selenite, and polarizing apparatus rotating by clockwork.

Mr. Hogg: Merz's immersion $\frac{1}{18}$th, exhibiting its penetration through thick covering glass, and its capability of being used with Wenham's binocular, with equal illumination of field three-fourths of each field space. Volvox and Closterium were shown in a cell deep enough to allow the rolling motion of the former, and yet not preventing the circulation in the latter, being displayed. Also scales of azure blue, attached to membrane by bulbous extremity, and torrefied Podura.

Mr. Charles Stewart: Injected bile ducts, showing terminal plexuses, from the liver of a cat; and larva case of some Trichopterous insect, interesting for so closely resembling a Hydra as to be readily taken for that animal. It is found in great abundance in Devonshire streams.

Mr. B. T. Lowne: Injected tissues from the frog, and dissections from the nervous system of the fly.

Donations to the Library and Cabinet from April 5th to May 3rd, 1871:—

	From
Land and Water. Weekly	*The Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*W. W. R.*
Journal of the Quekett Club, No. 14	*Club.*
Journal of the London Institution, No. 5	*Institution.*
Mikrographische Beitrage von Alexander Stuart in Odessa	*Author.*
Uber den Bau der Gregarinen; von Alexander Stuart in Odessa	*Author.*
Verhandlungen der Kaiserlich-koniglichen Zoologisch-Botanischen Gesellschaft, 1870.	
Klimatologische Notizen über den Winter in Hochgeberge. By Arthur W. Waters, F.R.S.	*Author.*
Traité du Microscope. Par Chas. Robin, 1871	*Author.*
Report on Barracks and Hospitals in America	*Surgeon-General, U.S.*
100 Slides of Vegetable Fibres	*W. T. Suffolk, Esq.*

The Rev. W. H. Dallinger and Benjamin Thompson Lowne, Esq., M.R.C.S., were elected Fellows of the Society.

WALTER W. REEVES,
Assist.-Secretary.

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